SCIENCE LECTURES FOR THE PEOPLE.

SCIENCE LECTURES

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PREFACE TO THE FOURTH SERIES.

THE Fourth Series of Manchester Science Lectures for the People has proved to be no less satisfactory than the preceding courses. Nearly 9,000 persons attended the ten lectures which have been delivered this season, whilst the interest exhibited has been undiminished, as shown both by the attention and magnitude of the audiences, and by the large demand for the penny publications.

Those attending the lectures have not only to thank the gentlemen who have this year spoken to them on the particular branches of science with which they are familiar, but also those who kindly subscribed the funds, without which the course could not have been carried on.

H. E. ROSCOE.

March, 1873.

GENERAL INDEX.

ON YEAST

A LECTURE

BY PROFESSOR HUXLEY, LL.D., F.R.S.

Delivered in the Free Trade Hall, Manchester, 3rd November, 1871.

I HAVE selected to-night the particular subject of Yeast for two reasons—or, rather, I should say for three. In the first place, because it is one of the simplest and the most familiar objects with which we are acquainted. In the second place, because the facts and phenomena which I have to describe are so simple that it is possible to put them before you without the help of any of those pictures or diagrams which are needed when matters are more complicated, and which, if I had to refer to them here, would involve the necessity of my turning away from you now and then, and thereby increasing very largely my difficulty (already sufficiently great) in making myself heard. And thirdly, I have chosen this subject because I know of no familiar substance forming part of our every day knowledge and experience, the examination of which, with a little care, tends to open up such very considerable issues as does this substance—yeast.

In the first place, I should like to call your attention to a fact with which the whole of you are, to begin with, perfectly acquainted, I mean the fact that any liquid containing sugar, any liquid which is formed by pressing out the succulent parts of the fruits of plants, or a mixture of honey and water, if left of itself for a short time, begins to undergo a peculiar change. No matter how clear it might be at starting, yet after a few hours, or at most a few days, if the temperature is high, this liquid begins to be turbid,

and by-and-by bubbles make their appearance in it, and a sort of dirty-looking yellowish foam or scum collects at the surface; while at the same time, by degrees, a similar kind of matter, which we call the "lees," sinks to the bottom.

The quantity of this dirty-looking stuff, that we call the scum and the lees, goes on increasing until it reaches a certain amount, and then it stops; and by the time it stops, you find the liquid in which this matter has been formed has become altered in its quality. To begin with it was a mere sweetish substance, having the flavour of whatever might be the plant from which it was expressed, or having merely the taste and the absence of smell of a solution of sugar; but by the time that this change that I have been briefly describing to you is accomplished the liquid has become completely altered, it has acquired a peculiar smell, and, what is still more remarkable, it has gained the property of intoxicating the person who drinks it. Nothing can be more innocent than a solution of sugar; nothing can be less innocent, if taken in excess, as you all know, than those fermented matters which are produced from sugar. Well, again, if you notice that bubbling, or, as it were, seething of the liquid, which has accompanied the whole of this process, you will find that it is produced by the evolution of little bubbles of air-like substance out of the liquid; and I dare say you all know this air-like substance is not like common air; it is not a substance which a man can breathe with impunity. You often hear of accidents which take place in brewers' vats when men go in carelessly, and get suffocated there without knowing that there was anything evil awaiting them. And if you tried the experiment with this liquid I am telling of while it was fermenting, you would find that any small animal let down into the vessel would be similarly stifled; and you would discover that a light lowered down into it would go out. Well, then, lastly, if after this liquid has been thus altered you expose it to that process which is called distillation; that is to say, if you put it into a still, and collect the matters which are sent over, you obtain, when you first heat it, a clear transparent liquid, which, however, is something totally different from water; it is much lighter; it has a strong smell, and it has an acrid taste; and it possesses the same intoxicating power as the original liquid, but in a much more intense degree. If you put a light to it, it burns with a bright flame, and it is that substance which we know as spirits of wine.

Now these facts which I have just put before you—all but the last—have been known from extremely remote antiquity. It is, I hope, one of the best evidences of the antiquity of the human

race, that among the earliest records of all kinds of men, you find a time recorded when they got drunk. We may hope that that must have been a very late period in their history. Not only have we the record of what happened to Noah, but if we turn to the traditions of a different people, those forefathers of ours who lived in the high lands of Northern India, we find that they were not less addicted to intoxicating liquids; and I have no doubt that the knowledge of this process extends far beyond the limits of historically recorded time. And it is a very curious thing to observe that all the names we have of this process, and all that belongs to it, are names that have their roots not in our present language, but in those older languages which go back to the times at which this country was peopled. That word "fermentation" for example, which is the title we apply to the whole process, is a Latin term; and a term which is evidently based upon the fact of the effervescence of the liquid. Then the French, who are very fond of calling themselves a Latin race, have a particular word for ferment, which is *levûre*. And, in the same way, we have the word "leaven," those two words having reference to the heaving up, or to the raising of the substance which is fermented. Now those are words which we get from what I may call the Latin side of our parentage; but if we turn to the Saxon side, there are a number of names connected with this process of fermentation. For example, the Germans call fermentation—and the old Germans did so—"*gähren;*" and they call anything which is used as a ferment by such names, such as "*ghest*" and "*gest*," and finally in low German, "*yest;*" and that word you know is the word our Saxon forefathers used, and is almost the same as the word which is commonly employed in this country to denote the common ferment of which I have been speaking. So they have another name, the word "*hefe*," which is derived from their verb "*heben*," which signifies to raise up; and they have yet a third name, which is also one common in this country (I do not know whether it is common in Lancashire, but it is certainly very common in the Midland counties), the word "*barm*," which is derived from a root which signifies to raise or to bear up. Barm is a something borne up; and thus there is much more real relation than is commonly supposed by those who make puns, between the beer which a man takes down his throat and the bier upon which that process, if carried to excess, generally lands him, for they are both derived from the root signifying bearing up; the one thing is borne upon men's shoulders, and the other is the fermented liquid which was borne up by the fermentation taking place in itself.

Again, I spoke of the produce of fermentation as "spirit of wine." Now what a very curious phrase that is, if you come to think of it. The old alchemists talked of the finest essence of anything as if it had the same sort of relation to the thing itself as a man's spirit is supposed to have to his body; and so they spoke of this fine essence of the fermented liquid as being the spirit of the liquid. Thus came about that extraordinary ambiguity of language, in virtue of which you apply precisely the same substantive name to the soul of man and to a glass of gin! And then there is still yet one other most curious piece of nomenclature connected with this matter, and that is the word "alcohol" itself, which is now so familiar to everybody. Alcohol originally meant a very fine powder. The women of the Arabs and other Eastern people are in the habit of tinging their eyelashes with a very fine black powder which is made of antimony, and they call that "kohol;" and the "al" is simply the article put in front of it, so as to say "the kohol." And up to the 17th century in this country the word alcohol was employed to signify any very fine powder; you find in Robert Boyle's works that he uses "alcohol" for a very fine subtle powder. But then this name of anything very fine and very subtle came to be specially connected with the fine and subtle spirit obtained from the fermentation of sugar; and I believe that the first person who fairly fixed it as the proper name of what we now commonly call spirits of wine, was the great French chemist Lavoisier, so comparatively recent is the use of the word alcohol in this specialised sense.

So much by way of general introduction to the subject on which I have to speak to-night. What I have hitherto stated is simply what we may call common knowledge, which everybody may acquaint himself with. And you know that what we call scientific knowledge is not any kind of conjuration, as people sometimes suppose, but it is simply the application of the same principles of common sense that we apply to common knowledge, carried out, if I may so speak, to knowledge which is uncommon. And all that we know now of this substance, yeast, and all the very strange issues to which that knowledge has led us, have simply come out of the inveterate habit, and a very fortunate habit for the human race it is, which scientific men have of not being content until they have routed out all the different chains and connections of apparently simple phenomena, until they have taken them to pieces and understood the conditions upon which they depend. I will try to point out to you now what has happened in conse-

quence of endeavouring to apply this process of "analysis," as we call it, this teazing out of an apparently simple fact into all the little facts of which it is made up, to the ascertained facts relating to the barm or the yeast; secondly, what has come of the attempt to ascertain distinctly what is the nature of the products which are produced by fermentation; then what has come of the attempt to understand the relation between the yeast and the products; and lastly, what very curious side issues—if I may so call them—have branched out in the course of this inquiry, which has now occupied somewhere about two centuries.

The first thing was to make out precisely and clearly what was the nature of this substance, this apparently mere scum and mud that we call yeast. And that was first commenced seriously by a wonderful old Dutchman of the name of Leeuwenhoek, who lived some two hundred years ago, and who was the first person to invent thoroughly trustworthy microscopes of high powers. Now, Leeuwenhoek went to work upon this yeast mud, and by applying to it high powers of the microscope, he discovered that it was no mere mud such as you might at first suppose, but that it was a substance made up of an enormous multitude of minute grains, each of which had just as definite a form as if it were a grain of corn, although it was vastly smaller, the largest of these not being more than the two-thousandth of an inch in diameter; while, as you know, a grain of corn is a large thing, and the very smallest of these particles were not more than the seven-thousandth of an inch in diameter. Leeuwenhoek saw that this muddy stuff was in reality a liquid, in which there were floating this immense number of definitely shaped particles, all aggregated in heaps and lumps and some of them separate. That discovery remained, so to speak, dormant for fully a century, and then the question was taken up by a French discoverer, who, paying great attention and having the advantage of better instruments than Leeuwenhoek had, watched these things and made the astounding discovery that they were bodies which were constantly being reproduced and growing; that when one of these rounded bodies was once formed and had grown to its full size, it immediately began to give off a little bud from one side, and then that bud grew out until it had attained the full size of the first, and that, in this way, the yeast particle was undergoing a process of multiplication by budding, just as effectual and just as complete as the process of multiplication of a plant by budding; and thus this Frenchman, Cagniard de la Tour, arrived at the conclusion—very creditable to his sagacity, and which has been confirmed by every observation and reasoning since—that this apparently muddy

refuse was neither more nor less than a mass of plants, of minute living plants, growing and multiplying in the sugary fluid in which the yeast is formed. And from that time forth we have known this substance which forms the scum and the lees as the yeast plant; and it has received a scientific name—which I may use without thinking of it, and which I will therefore give you—namely, "Torula." Well, this was a capital discovery. The next thing to do was to make out how this torula was related to other plants. I won't weary you with the whole course of investigation, but I may sum up its results, and they are these—that the torula is a particular kind of a fungus, a particular state rather, of a fungus or mould. There are many moulds which under certain conditions give rise to this torula condition, to a substance which is not distinguishable from yeast, and which has the same properties as yeast—that is to say, which is able to decompose sugar in the curious way that we shall consider by-and-by. So that the yeast plant is a plant belonging to a group of the Fungi, multiplying and growing and living in this very remarkable manner in the sugary fluid which is, so to speak, the nidus or home of the yeast.

That, in a few words, is, as far as investigation—by the help of one's eye and by the help of the microscope—has taken us. But now there is an observer whose methods of observation are more refined than those of men who use their eye, even though it be aided by the microscope; a man who sees indirectly further than we can see directly—that is, the chemist; and the chemist took up this question, and his discovery was not less remarkable than that of the microscopist. The chemist discovered that the yeast plant being composed of a sort of bag, like a bladder, inside which is a peculiar soft, semifluid material—the chemist found that this outer bladder has the same composition as the substance of wood, that material which is called "cellulose," and which consists of the elements carbon and hydrogen and oxygen, without any nitrogen But then he also found (the first person to discover it was an Italian chemist, named Fabroni, in the end of the last century) that this inner matter which was contained in the bag, which constitutes the yeast plant, was a substance containing the elements carbon and hydrogen and oxygen and nitrogen; that it was what Fabroni called a vegeto-animal substance, and that it had the peculiarities of what are commonly called "animal products."

This again was an exceedingly remarkable discovery. It lay neglected for a time, until it was subsequently taken up by the great chemists of modern times, and they, with their delicate

methods of analysis, have finally decided that, in all essential respects, the substance which forms the chief part of the contents of the yeast plant is identical with the material which forms the chief part of our own muscles, which forms the chief part of our own blood, which forms the chief part of the white of the egg; that, in fact, although this little organism is a plant, and nothing but a plant, yet that its active living contents contain a substance which is called "protein," which is of the same nature as the substance which forms the foundation of every animal organism whatever.

Now we come next to the question of the analysis of the products, of that which is produced during the process of fermentation. So far back as the beginning of the 16th century, in the times of transition between the old alchemy and the modern hemistry, there was a remarkable man, Von Helmont, a Dutchman, who saw the difference between the air which comes out of a vat where something is fermenting and common air. He was the man who invented the term "gas," and he called this kind of gas "*gas silvestre*"—so to speak gas that is wild, and lives in out of the way places—having in his mind the identity of this particular kind of air with that which is found in some caves and cellars. Then, the gradual process of investigation going on, it was discovered that this substance, then called "fixed air," was poisonous gas, and it was finally identified with that kind of gas which is obtained by burning charcoal in the air, which is called "carbonic acid." Then the substance alcohol was subjected to examination, and it was found to be a combination of carbon, and hydrogen, and oxygen. Then the sugar which was contained in the fermenting liquid was examined, and that was found to contain the three elements carbon, hydrogen, and oxygen. So that it was clear there were in sugar the fundamental elements which are contained in the carbonic acid, and in the alcohol. And then came that great chemist Lavoisier, and he examined into the subject carefully, and possessed with that brilliant thought of his which happens to be propounded exactly apropos to this matter of fermentation—that no matter is ever lost, but that matter only changes its form and changes its combinations—he endeavoured to make out what became of the sugar which was subjected to fermentation. He thought he discovered that the whole weight of the sugar was represented by the weight of the alcohol produced, added to the weight of the carbonic acid produced; that in other words, supposing this tumbler to represent the sugar, that the action of fermentation was as it were the splitting of it,

the one half going away in the shape of carbonic acid, and the other half going away in the shape of alcohol. Subsequent inquiry, careful research with the refinements of modern chemistry, have been applied to this problem, and they have shown that Lavoisier was not quite correct; that what he says is quite true for about 95 per cent of the sugar, but that the other 5 per cent, or nearly so, is converted into two other things; one of them, matter which is called succinic acid, and the other matter which is called glycerine, which you all know now as one of the commonest of household matters. It may be that we have not got to the end of this refined analysis yet, but at any rate, I suppose I may say—and I speak with some little hesitation for fear my friend Professor Roscoe here may pick me up for trespassing upon his province—but I believe I may say that now we can account for 99 per cent at least of the sugar, and that that 99 per cent is split up into these four things, carbonic acid, alcohol, succinic acid, and glycerine. So that it may be that none of the sugar whatever disappears, and that only its parts, so to speak, are re-arranged, and if any of it disappears, certainly it is a very small portion.

Now these are the facts of the case. There is the fact of the growth of the yeast plant; and there is the fact of the splitting up of the sugar. What relation have these two facts to one another?

For a very long time that was a great matter of dispute. The early French observers, to do them justice, discerned the real state of the case, namely, that there was a very close connection between the actual life of the yeast plant and this operation of the splitting up of the sugar; and that one was in some way or other connected with the other. All investigation subsequently has confirmed this original idea. It has been shown that if you take any measures by which other plants of like kind to the torula would be killed, and by which the yeast plant is killed, then the yeast loses its efficiency. But a capital experiment upon this subject was made by a very distinguished man, Helmholz, who performed an experiment of this kind. He had two vessels—one of them we will suppose full of yeast, but over the bottom of it, as this might be, was tied a thin film of bladder; consequently, through that thin film of bladder all the liquid parts of the yeast would go, but the solid parts would be stopped behind; the torula would be stopped, the liquid parts of the yeast would go. And then he took another vessel containing a fermentable solution of sugar, and he put one inside the other; and in this way you see the fluid parts of the yeast were able to pass through with the utmost ease into the sugar, but the solid

parts could not get through at all. And he judged thus: if the fluid parts are those which excite fermentation, then, inasmuch as these are stopped, the sugar will not ferment; and the sugar did not ferment, showing quite clearly that an immediate contact with the solid, living torula was absolutely necessary to excite this process of splitting up of the sugar. This experiment was quite conclusive as to this particular point, and has had very great fruits in other directions.

Well, then, the yeast plant being essential to the production of fermentation, where does the yeast plant come from? Here, again, was another great problem opened up, for, as I said at starting, you have, under ordinary circumstances in warm weather, merely to expose some fluid containing a solution of sugar, or any form of syrup or vegetable juice to the air, in order, after a comparatively short time, to see all these phenomena of fermentation. Of course the first obvious suggestion is, that the torula has been generated within the fluid. In fact, it seems at first quite absurd to entertain any other conviction; but that belief would most assuredly be an erroneous one.

Towards the beginning of this century, in the vigorous times of the old French wars, there was a Monsieur Appert, who had his attention directed to the preservation of things that ordinarily perish, such as meats and vegetables, and in fact he laid the foundation of our modern method of preserving meats; and he found that if he boiled any of these substances and then tied them so as to exclude the air, that they would be preserved for any time. He tried these experiments, particularly with the must of wine and with the wort of beer; and he found that if the wort of beer had been carefully boiled and was stopped in such a way that the air could not get at it, it would never ferment. What was the reason of this? That, again, became the subject of a long string of experiments, with this ultimate result, that if you take precautions to prevent any solid matters from getting into the must of wine or the wort of beer, under these circumstances—that is to say, if the fluid has been boiled and placed in a bottle, and if you stuff the neck of the bottle full of cotton wool, which allows the air to go through, and stops anything of a solid character however fine, then you may let it be for ten years and it will not ferment. But if you take that plug out and give the air free access, then, sooner or later, fermentation will set up. And there is no doubt whatever that fermentation is excited only by the presence of some torula or other, and that that torula proceeds, in our present experience, from pre-existing

torulæ. These little bodies are excessively light. You can easily imagine what must be the weight of little particles, but slightly heavier than water, and not more than the two thousandth or perhaps seven thousandth of an inch in diameter. They are capable of floating about and dancing like motes in the sunbeam; they are carried about by all sorts of currents of air; the great majority of them perish; but one or two, which may chance to enter into a sugary solution, immediately enter into active life, find there the conditions of their nourishment, increase and multiply, and may give rise to any quantity whatever of this substance yeast. And, whatever may be true or not be true about this "spontaneous generation," as it is called, in regard to all other kinds of living things, it is perfectly certain, as regards yeast, that it always owes its origin to this process of transportation or inoculation, if you like so to call it, from some other living yeast organism; and so far as yeast is concerned, the doctrine of spontaneous generation is absolutely out of court. And not only so, but the yeast must be alive in order to exert these peculiar properties. If it be crushed, if it be heated so far that its life is destroyed, that peculiar power of fermentation is not excited. Thus we have come to this conclusion, as the result of our inquiry, that the fermentation of sugar, the splitting of the sugar into alcohol and carbonic acid, glycerine, and succinic acid, is the result of nothing but the vital activity of this little fungus, the torula.

And now comes the further exceedingly difficult inquiry—how is it that this plant, the torula, produces this singular operation of the splitting up of the sugar? Fabroni, to whom I referred some time ago, imagined that the effervescence of fermentation was produced in just the same way as the effervescence of a seidlitz powder, that the yeast was a kind of acid, and that the sugar was a combination of carbonic acid and some base to form the alcohol, and that the yeast combined with this substance, and set free the carbonic acid; just as when you add carbonate of soda to acid you turn out the carbonic acid. But of course the discovery of Lavoisier that the carbonic acid and the alcohol taken together are very nearly equal in weight to the sugar, completely upset this hypothesis. Another view was therefore taken by the French chemist, Thénard, and it is still held by a very eminent chemist, M. Pasteur, and their view is this, that the yeast, so to speak, eats a little of the sugar, turns a little of it to its own purposes, and by so doing gives such a shape to the sugar that the rest of it breaks up into carbonic acid and alcohol.

Well, then, there is a third hypothesis, which is maintained by

another very distinguished chemist, Liebig, which denies either of the other two, and which declares that the particles of the sugar are, as it were, shaken asunder by the forces at work in the yeast plant. Now I am not going to take you into these refinements of chemical theory, I cannot for a moment pretend to do so, but I may put the case before you by an analogy. Suppose you compare the sugar to a card house, and suppose you compare the yeast to a child coming near the card house, then Fabroni's hypothesis was that the child took half the cards away; Thénard's and Pasteur's hypothesis is that the child pulls out the bottom card and thus makes it tumble to pieces; and Liebig's hypothesis is that the child comes by and shakes the table and tumbles the house down. I appeal to my friend here (Professor Roscoe) whether that is not a fair statement of the case.

Having thus, as far as I can, discussed the general state of the question, it remains only that I should speak of some of those collateral results which have come in a very remarkable way out of the investigation of yeast. I told you that it was very early observed that the yeast plant consisted of a bag made up of the same material as that which composes wood, and of an interior semifluid mass which contains a substance, identical in its composition, in a broad sense, with that which constitutes the flesh of animals. Subsequently, after the structure of the yeast plant had been carefully observed, it was discovered that all plants, high and low, are made up of separate bags or "cells," as they are called; these bags or cells having the composition of the pure matter of wood; having the same composition, broadly speaking, as the sac of the yeast plant, and having in their interior a more or less fluid substance containing a matter of the same nature as the protein substance of the yeast plant. And therefore this remarkable result came out—that however much a plant may differ from an animal, yet that the essential constituent of the contents of these various cells or sacs of which the plant is made up, the nitrogenous protein matter, is the same in the animal as in the plant. And not only was this gradually discovered, but it was found that these semifluid contents of the plant cell had, in many cases, a remarkable power of contractility quite like that of the substance of animals. And about 24 or 25 years ago, namely, about the year 1846, to the best of my recollection, a very eminent German botanist, Hugo Von Mohl, conferred upon this substance which is found in the interior of the plant cell, and which is identical with the matter found in the

inside of the yeast cell, and which again contains an animal substance similar to that of which we ourselves are made up—he conferred upon this that title of "protoplasm," which has brought other people a great deal of trouble since! I beg particularly to say that, because I find many people suppose that I was the inventor of that term, whereas it has been in existence for at least twenty-five years. And then other observers, taking the question up, came to this astonishing conclusion (working from this basis of the yeast), that the differences between animals and plants are not so much in the fundamental substances which compose them, not in the protoplasm, but in the manner in which the cells of which their bodies are built up have become modified. There is a sense in which it is true—and the analogy was pointed out very many years ago by some French botanists and chemists—there is a sense in which it is true that every plant is substantially an enormous aggregation of bodies similar to yeast cells, each having to a certain extent its own independent life. And there is a sense in which it is also perfectly true—although it would be impossible for me to give the statement to you with proper qualifications and limitations on an occasion like this—but there is also a sense in which it is true that every animal body is made up of an aggregation of minute particles of protoplasm, comparable each of them to the individual separate yeast plant. And those who are acquainted with the history of the wonderful revolution which has been worked in our whole conception of these matters in the last thirty years, will bear me out in saying that the first germ of them, to a very great extent, was made to grow and fructify by the study of the yeast plant, which presents us with living matter in almost its simplest condition.

Then there is yet one last and most important bearing of this yeast question. There is one direction probably in which the effects of the careful study of the nature of fermentation will yield results more practically valuable to mankind than any other. Let me recall to your minds the fact which I stated at the beginning of this lecture. Suppose that I had here a solution of pure sugar with a little mineral matter in it; and suppose it were possible for me to take upon the point of a needle one single, solitary yeast cell, measuring no more perhaps than the three thousandth of an inch in diameter—not bigger than one of those little coloured specks of matter in my own blood at this moment, the weight of which it would be difficult to express in the fraction of a grain—and put it into this solution. From that single one, if the solution were kept at a fair temperature in a

warm summer's day, there would be generated, in the course of a week, enough torulæ to form a scum at the top and to form lees at the bottom, and to change the perfectly tasteless and entirely harmless fluid, syrup, into a solution impregnated with the poisonous gas carbonic acid, impregnated with the poisonous substance alcohol; and that, in virtue of the changes worked upon the sugar by the vital activity of these infinitesimally small plants. Now you see that this is a case of infection. And from the time that the phenomenon of fermentation were first carefully studied, it has constantly been suggested to the minds of thoughtful physicians that there was a something astoundingly similar between this phenomena of the propagation of fermentation by infection and contagion, and the phenomena of the propagation of diseases by infection and contagion. Out of this suggestion has grown that remarkable theory of many diseases which has been called the "germ theory of disease," the idea, in fact, that we owe a great many diseases to particles having a certain life of their own, and which are capable of being transmitted from one living being to another, exactly as the yeast plant is capable of being transmitted from one tumbler of saccharine substance to another. And that is a perfectly tenable hypothesis, one which in the present state of medicine ought to be absolutely exhausted and shown not to be true, until we take to others which have less analogy in their favour. And there are some diseases most assuredly in which it turns out to be perfectly correct. There are some forms of what are called malignant carbuncle which have been shown to be actually effected by a sort of fermentation, if I may use the phrase, by a sort of disturbance and destruction of the fluids of the animal body, set up by minute organisms which are the cause of this destruction and of this disturbance; and only recently the study of the phenomena which accompany vaccination has thrown an immense light in this direction, tending to show by experiments of the same general character as that to which I referred as performed by Helmholz, that there is a most astonishing analogy between the contagion of that healing disease and the contagion of destructive diseases. For it has been made out quite clearly, by investigations carried on in France and in this country, that the only part of the vaccine matter which is contagious, which is capable of carrying on its influence in the organism of the child who is vaccinated, is the solid particles and not the fluid. By experiments of the most ingenious kind, the solid parts have been separated from the fluid parts, and it has then been discovered that you may vaccinate a child as much as you

like with the fluid parts, but no effect takes place, though an excessively small portion of the solid particles, the most minute that can be separated, is amply sufficient to give rise to all the phenomena of the cow pock, by a process which we can compare to nothing but the transmission of fermentation from one vessel into another, by the transport to the one of the torula particles which exist in the other. And it has been shown to be true of some of the most destructive diseases which infect animals, such diseases as the sheep pox, such diseases as that most terrible and destructive disorder of horses, glanders, that in these, also, the active power is the living solid particle, and that the inert part is the fluid. However, do not suppose that I am pushing the analogy too far. I do not mean to say that the active, solid parts in these diseased matters are of the same nature as living yeast plants; but, so far as it goes, there is a most surprising analogy between the two; and the value of the analogy is this, that by following it out we may some time or other come to understand how these diseases are propagated, just as we understand, now, about fermentation; and that, in this way, some of the greatest scourges which afflict the human race may be, if not prevented, at least largely alleviated.

This is the conclusion of the statements which I wished to put before you. You see we have not been able to have any accessories. If you will come in such numbers to hear a lecture of this kind, all I can say is, that diagrams cannot be made big enough for you, and that it is not possible to show any experiments illustrative of a lecture on such a subject as I have to deal with. Of course my friends the chemists and physicists are very much better off, because they can not only show you experiments, but you can smell them and hear them! But in my case such aids are not attainable, and therefore I have taken a simple subject and have dealt with it in such a way that I hope you all understand it, at least so far as I have been able to put it before you in words; and having once apprehended such of the ideas and simple facts of the case as it was possible to put before you, you can see for yourselves the great and wonderful issues of such an apparently homely subject.

ON COAL COLOURS.

A LECTURE,

BY PROFESSOR ROSCOE, F.R.S.,

Delivered in the Hulme Town Hall. November 18th, 1871.

THE subject of coal has naturally attracted much of our attention in these Science Lectures. In the first series, Professor Jevons, than whom no one in the country is more able to speak upon the economic aspects of the question, discoursed of the importance of coal in manufactures and trades; whilst in the last series Mr. Boyd Dawkins and Mr. Green unfolded some of the secrets which lie hidden in a piece of coal. I propose to take up the subject this evening from another point of view, and to endeavour to open out to you still more wonderful, and, if possible, still more interesting fields than they did, inasmuch as I shall attempt to give you an account of the composition of coal, and of one or two of the very large number of derivatives which we can obtain from coal.

You are all aware that from coal we get the magnificent colours which are so much admired, and which are used so much in silk, woollen, and cotton dyeing. You know also, perhaps, that even certain essences and sweet savours can be obtained from this dirty-looking substance—a piece of coal.

To tell you all about the bodies which have been got from coal would take me a very long time, I therefore only propose to give you a short history of the mode in which these bodies are obtained, choosing out one or two for our more special study.

In order to commence the study of our subject, I will, in the first place, take here two tobacco pipes, in each of which I have placed a small quantity of coal. In the one I have placed a small quantity of the kind of coal which is found in South Wales, and which is called anthracite coal; whilst in the other pipe we have placed

some coal which is found at Wigan, and is called cannel coal. The difference between the effect of heat upon these two kinds of coal will very soon be visible to you. We shall be able to get from the pipe in which we have placed the cannel coal a quantity of brown vapour, which on bringing a light to it will take fire; whilst from the other pipe we shall not get any such brown vapour at all. Now this shows us at once that coals differ very widely in their properties.

Coal, as you have been told in the previous lectures, is a body made up of several elementary constituents. It contains carbon, hydrogen, nitrogen, and oxygen; and the quantities of these elements which the coals contain varies very much. In this cannel coal there is a much larger quantity or proportion of hydrogen and oxygen than there is in the anthracite coal. There is much more of what we call volatile or bituminous matter; and therefore this cannel coal will yield us a much larger quantity of gas than can be got by the use of anthracite coal. Anthracite coal is almost pure carbon.

[The experiment with the coal in the pipes and all the subsequent experiments were very successful, and were much applauded.]

The quantity of gas or volatile products which can be obtained from different kinds of coal depends in the first place, then, upon the composition of the coal. I have here a small model of a gas making apparatus; in which the same process is going on which occurs in an enormously larger scale in the gas works of the Corporation of Manchester. And for this purpose I have used cannel coal, because the anthracite coal does not yield us any supply of gas. Let us now examine what takes place in the gas works—what is going on when we make this coal gas. We may divide the products of the gas works into four classes:—first, the coke, which is left behind in the retort; secondly, the gas which comes off; thirdly, the watery liquid which is formed; and fourthly, the tarry matter which comes with the gas, but which, together with the watery liquid, is not sent through the mains, but is condensed before it leaves the gas works.

Let us now notice what is the chemical composition, first of the coal gas itself; secondly, of the watery portions, called the ammonia water; and thirdly, of the gas tar. On the side of the room I have suspended a large diagram of the various products of coal, some of them having rather curious names (see Table on page 5). I am afraid that it may frighten some of you if you think that I am going to talk about all these substances. I do not intend

to do so; but I wish you to see what a very large number of chemical substances exist as the products of the destructive distillation of coal. Mark the words "destructive distillation," because I shall have to speak of this again. In the destruction of the coal by distillation, all these products can be got, and are found either in the gas or in the coke, or in the ammonia liquor, or in the tar.

Here I have two pounds of cannel coal. I have here a large white cube, each of whose sides is 26 inches in length, which represents the quantity of gas which can be got from these two pounds of cannel coal. I have in this bottle the exact quantity of coke, namely, 19 ounces, which would be left behind in the retort when this quantity of coal is heated. Here is three ounces of watery ammonia liquor which would come away; and this is the 2¾ ounces of tar which would be formed by the destructive distillation of two pounds of coal. You will see from the diagram below that 100 tons of cannel coal distilled to yield 10.000 cubic feet of gas, having a specific gravity of 0·6, gives the following products: about 60 tons of coke, 9½ tons of ammonia water, 8½ of tar, and 22¼ of gas, by weight. This expresses in numbers what you there see illustrated by the model.

DESTRUCTIVE DISTILLATION OF COAL.

100 tons of cannel coal distilled to yield 10,000 cubic feet of gas of specific gravity, 0·6, gives the following products:

	GAS.	TAR.	AMMONIA WATER.	COKE.	SOURCE.
1	22·25	8·5	9·5	59·75	Average (Muspratt)
2	20·01	7·85	7·14	65·00	Manchester
3	20·40	6·10	5·40	67·84	Dukinfield
4	21·70	7·50	5·80	65·00	Macclesfield
5	16·50	10·70	8·00	65·00	

First, then, with regard to the gas. Coal gas—that with which we are supplied and lighted at the present time—is not one definite chemical compound, but is a mixture of several component chemical substances, and the composition of coal gas varies very much. Here in the north of England we get a better gas than those who live in the south, because here we have the command of a better sort of cannel coal. In London the ordinary illuminating power of the gas is about 12½ candles; whilst in Manchester the gas has an illuminating power of about 20 candles; that is, a jet of gas burning at the rate of 5 cubic feet per hour gives a light equal to

that given by 20 standard candles. I mention this to show that gas is not the same all the world over, but that it depends both upon the quality of the coal employed, and upon the mode of its manufacture.

Now the substances which coal gas contains may be divided into three classes; first, those parts of the gas which give off light, or the illuminating constituents; secondly, those parts of the gas which burn, but which do not give off light, and which may be termed heating constituents; and thirdly, those portions of the gas which neither give off light nor heat, that is to say, which do not burn at all, and these may be termed the impurities contained in the gas, which require to be removed, or ought to be removed completely in the process of gas making, and before the gas is distributed to the town. Here we have one of the luminous constituents of coal gas. This is termed ethylene or olefiant gas. You see it burns with a very bright and brilliant light. This is the chief illuminating constituent of coal gas. Here we have another constituent of coal gas, termed carbonic oxide gas, which burns with a very pale blue flame, as you will observe, but which scarcely gives off any light. This is one of the heating constituents of the coal gas or diluents, as they have been termed, because they dilute the illuminating constituents. Here we have another constituent which requires removal from the coal gas, namely, carbonic acid gas; and this you see extinguishes the taper the moment I place it in the gas. This, together with sulphuretted hydrogen and the vapour of bisulphide of carbon, ought to be removed in the process of gas making, and this is more or less completely done by the scrubbers and the lime—or oxide of iron—purifiers. In the following table you will see first the names of the three illuminating constituents; the next four are the heating constituents; and the next three are the impurities which have to be removed.

We have here an arrangement for making gas: the fire is burning and heating the cannel coal contained in this iron retort; here is what is termed the tar well, for the first thing that is deposited from the heated gas when it cools is the tar. These tubes are termed atmospheric condensers, where the gas is cooled and more of the tar deposited; and here we have the purifiers for the purpose of ridding the gas of the three impurities to which I have referred; and here we have the gas holder, into which the gas is now passing, and from which we can now pass it through our system of mains and light it, as you see here. [Gas made in the room was then ignited.]

Now, passing down the list, the next material we reach is the ammonia water.

PRODUCTS FOUND IN THE DESTRUCTIVE DISTILLATION OF COAL.

COAL GAS.

Ethylene, Tritylene, Tetralene, } Illuminating constituents.

Marshgas, Acetylene, Carbonic oxide, Hydrogen; } Diluents or heating constituents.

Carbonic acid, Sulphuretted hydrogen, Carbon dishulphide, } Impu

AMMONIA WATER.

TAR-PITCH.

COAL-TAR.

Paraffines.

Amyl hydride.
Hexyl hydride.
Heptyl hydride.
Octyl hydride.
Nonyl hydride.
Decatyl hydride.

Olefines.

Amylene.
Hexylene.
Heptylene.
Octylene.
Nonylene.
Decatylene.

Acetylene Series.

Terpenes.

Benzene Series.

Benzene.
Toluene.
Xylene.
Isoxylene.
Pseudo-cumene.
Mesitylene.

Napthaline.

Anthracene.

Pyrene.

Chrysene.

Phenols.

Phenol, or Carbolic Acid.
Cresol.
Xylenol.

Bases.

Aniline.
Tolindine, &c.
Pyridin.
Picolin.
Lutidin.
Collidin.
Parvolin.
Coridin.
Rubidin.
Viridin.

Leucolin.
Iridolin.
Cryptidin.

This ammonia water is a very important part of the gas products, because from this a number of very interesting substances are obtained. Now what is the ammonia water? The ammonia water is a liquid coming from the coal, for a good deal of moisture, which the coal contains, comes over with

the products, and this moisture condenses or absorbs the gas called ammonia, forming what I dare say most of you know as spirits of hartshorn. Now this gas-ammonia is a compound body, and contains nitrogen and hydrogen. The nitrogenous portion of the coal is converted in the process of distillation into this ammoniacal gas, which is taken hold of by the water, and the solution flows down as a brownish coloured, strongly smelling liquid, known as "gas water," which is pumped off and sold for purposes of manufacturing the ammoniacal salts and alum. We have here specimens of sal-ammoniac and of carbonate of ammoniac and also a large lump of alum, which I have to thank Mr. Spence for sending. All these substances are made from the ammonia liquor. Now I wish to show you that this ammonia gas which is given off will dissolve in water, and that is the reason why it does not come off with the rest of the gas, but is kept back as a liquid; in order to show that I will make a simple experiment: we have got here a large globe, filled with this gas ammonia, which as you see is a colourless, invisible gas, but possesses a very pungent smell, and has the power of dissolving very rapidly in water. Now in the lower vessel I have got some water, and I am going to blow a little of this reddened water up into this upper globe, filled with the ammoniacal gas, and you will see that the whole of this water will rush up into the upper globe, because the ammonia dissolves in the water, and the water therefore takes the place of the gas, and we shall have a very beautiful fountain produced. [Experiment very interesting and successful.] There now you see that the ammonia has been absorbed by the water, and the effect of the alkaline nature of this substance is seen, inasmuch as the red liquid has turned blue.

Now we get to the next part of our subject—the COAL-TAR, and the greater part of what I have to say will be with regard to the tar contained in the products of the distillation of coal. In the first place, with regard to the tar, let me say this, that we can obtain from tar a great variety of very beautiful white colourless substances. For instance, this white crystalline body here is carbolic acid, so largely used for disinfecting purposes; this beautiful white crystalline substance napthaline; this beautiful clear, colourless liquid benzole, all come from that dirty substance—coal tar—which you see, and which you rather avoid when you do see it, going along the streets in those very black, dirty-looking barrels. Nay, even from similar products of coal tar this beautiful white body—paraffin—can be got. It was the great chemist Liebig who some years ago said that the man who

should be able to liquify coal gas, so that it could be carried about readily from place to place, would be a great benefactor to his species. This has now been done, mainly through the labours of one man, Mr. James Young, who first began this conversion of coal into oil. These products of the distillation of coal are not obtained in gas making, it is true, but they are obtained by quite a similar process—the destructive distillation of a coal-like substance, at a lower temperature than that used for making coal gas.

It seems, I dare say, hard for you to understand how such a beautiful white body as this paraffin can be got from black coal. But I will show you a few experiments which I think will render this subject clearer to you. We have here a very well-known substance—sugar. This white sugar I will now dissolve in a little hot water, and I think in a few moments I can show you that this white sugar contains carbon. I am now going through the opposite process to that which is done by Mr. Young in distilling his shale. I am going to convert a white substance into carbon. The point I wish to illustrate is, that it is possible to get a white substance like paraffin from a black one as coal, inasmuch as the white substance contains carbon, only in a different state of combination. I have only got now to pour into this some strong sulphuric acid, when you will see that this sugar will be converted into charcoal. (The conversion into a seething, black, frothy substance was instantaneous.] Here you see that the whole of this white substance has been converted into charcoal. So much, then, for the fact that a white solid body contains carbon. I have in this bottle another colourless substance, liquid turpentine, and I wish to show you that turpentine also contains carbon. I will pour a little of this turpentine on to a bit of paper, and then plunge it into this cylinder of chlorine gas, when I think you will see that the carbon of the turpentine will become visible. [A cloud of black vapour is instantly produced.] In the same way I have got here a colourless olefiant gas, which also contains carbon, and when I mix this gas together with chlorine gas, and bring a light to the mixture, I get a large quantity of carbon set free, and thus we learn that white solids, colourless liquids, and colourless gases all may contain black carbon; and it must, therefore, not surprise you to find that from black coal we can get these beautiful white bodies.

What I have as yet said has reference to the destructive distillation of coal. I have had to destroy the coal in order to get these various new and interesting products. Let us now turn to another question, and let us ask ourselves, can we by

any other process than this destructive action get hold of new bodies? The first era in chemical science has been what we term the analytical era. By analysis we mean destruction, breaking up, pulling asunder. The first object that the chemist had to achieve was to find what he could get by destroying bodies. We have destroyed the coal, and we have got this variety of substances whose names you find on the list. The second era in chemical science is what we term the synthetic or constructive era, the era in which we begin to build up. We all know it is very much more easy to destroy than it is to construct. And as it is in every-day life, so it is with chemical compounds, as proved by the history of chemical science. It is very much more easy to find out what we can get by destroying the coal than it is to find out what we can make by building up the various substances which are obtained from coal. Hence it is, as you will easily understand, that analytical chemistry or destructive chemistry came first in the history of science, and then came synthetic chemistry.

Within the last forty years very great progress has been made in this constructive chemistry. Before the year 1828, it was generally supposed that any chemical substance which was found in animal or vegetable bodies (which substances you will understand are very numerous) was constructed in the body of the animal or plant, according to laws altogether different from the laws by which the chemist was able to build up what are termed his inorganic compounds. He could bring together oxygen and hydrogen, and form water; he could bring together sulphur and copper, and get a black sulphide; but could he obtain such a substance as urea, which was only found in the products of animal life? This was the great question. And this has, by dint of laborious experimental investigations, been answered most completely in the affirmative. He can construct the substances which are found in the bodies of animals and plants. He has not succeeded in constructing all these substances, but he has succeeded in constructing a great number. I might give you instances of hundreds of substances which were first known as products solely found in animal or vegetable bodies, but which have since been built up from their constituent elements. Thus, for instance, that curious acid has been produced which is found in the bodies of ants, and which we term formic acid, and which is also found in the sting of the nettle, the sting being due to the peculiar effect of this acrid liquid. This formic acid was originally found only in these two sources, but formic acid can now be procured from its organic constituents, from carbon, hydrogen, and oxygen. So too with alcohol, about which Professor

Huxley discoursed in his lecture on yeast, last week. He showed you that the process by which alcohol is ordinarily formed is a very complicated one, and one which it is altogether beyond the power of the chemist to follow. The chemist cannot tell you the exact process by which the yeast particles decompose the sugar and liberate the alcohol, carbonic acid, glycerine, succinic acid, and other products. That is a process not perhaps so completely dark to us as the processes which go on in the animal and vegetable bodies, but it is a process about which chemists know very little, and is doubtless a process analogous to those which go on in the living body. But this alcohol can now be built up from its elements, or from mineral constituents, from charcoal, hydrogen, and oxygen. And so I might go on with illustrations of substances which were supposed originally to be only the sole products of that action which is termed vital action, but which now we find can be formed in the ordinary way of chemical synthesis. For instance, only the other day the beautiful and singular substance known as essential essence of the Tonka bean was prepared artificially. Those persons who take snuff are very fond of carrying this bean in their snuff boxes, because it imparts to the snuff a still more pungent and agreeable odour. It is a white crystalline body, termed coumarine, and this has been quite recently prepared artificially, and found to possess all the properties of that contained in this peculiar bean. In short, as far as regards the artificial construction of liquid or crystalline products produced in vital processes, the chemist's power seems boundless, though, when we come to organised bodies—such as the yeast globule or the starch grain, our domain seems to end, for the chemist knows nothing about the artificial formation of the simplest organised structure.

Well, then, let us see what we can learn with regard to constructive chemistry as applied to the coal products. We shall find that the substances which can be artificially built up from the bodies contained in coal-tar possess most interesting properties; thus, for instance, they exhibit the most remarkable colouring powers.

In the year 1825, our great English philosopher Faraday discovered benzole. This benzole was then a chemical rarity; now it is prepared by thousands of tons for the production of the beautiful aniline colours which you know so well. From the crude benzole contained in the tar we can build up, by a process of addition, the details of which I have not time to describe to-night, this heavy liquid aniline; and this has the power, after it has been subjected to another additive process, of producing the most

beautiful colours. I have in this jar a small quantity of aniline; I will add a drop or two to the water in this large glass globe; and now I will add some of this colourless liquid, hypochlorite of sodium, and after a while you will see that the colour of this water will be changed, and that we shall have a splendidly violet-coloured liquid, containing the well-known colour, mauve, which was discovered by Mr. Perkin, in 1856, and this will give you an idea of the beauty of the colours which are got from coal. By a modification of the constructive processes to which the crude aniline is subjected a great variety of differently-coloured substances can be got thus. There we have the beautiful aniline blue colour. Here we have got the celebrated aniline red, known as magenta, and a bloody red it is. Here we have another coloured derivative—the aniline violet. In these compounds which we can thus build up we have not only a mine of interest, but also a mine of wealth, for the money value of these aniline colours is enormous. And how interesting it is to think that this body, aniline, which a few years ago was a curiosity, and only found in the laboratories of the chemists, is now a substance which is manufactured by tons, and thousands of tons, and which can be thus made to minister to our gratification, and appeal to our sense of beauty!

Another interesting point I must not forget to mention, and that is, that these beautiful colours are compounds of bodies which are perfectly colourless! Through the kindness of my friends, Messrs. Roberts, Dale, and Co., who are one of the largest manufacturers of these beautiful colours in England, I have here some of these bodies in their colourless state. Let me show you how these colourless bodies can be made to become brightly coloured. It is on combining these colourless bodies with acids that their colouring power first becomes evident. Here is a colourless liquid. I pour a little of it on to this piece of white blotting paper, and on warming the paper over a lamp a bright green colour becomes at once apparent. This is because the base of the green-coloured compound does not possess any colour whatever, and it is only when this base is by drying converted into a salt that the colour appears. Again, I take a colourless solution—rosaniline, and I have only to heat it to convert it into salt, and the beautiful bright red colour at once is seen. A very small quantity of this, placed on a piece of white paper, will, in a moment or two, when dried, turn the colourless paper into a bright crimson. This, then, is a very interesting and singular property of these colours. I may show it to you in another way. I will write on this large sheet of white calico, stretched on a

frame, the three words "blue," and "red," and "green," in large letters, with the colourless solutions of the bases, and then if I rub a little acid on the back of the paper you see that it instantly brings out these three colours. This illustrates the fact that the colour of a chemical substance, is not, as it were, an essential or necessary characteristic of it, the colour in this case depends upon an acid being present, for the pure bases of these colours are colourless.

Now, I might, if I had time, tell you much more respecting these splendid blue, red, and violet colours which are derived from the aniline. I will, however, now describe to you another and perhaps a still more interesting colouring matter, which has been more recently obtained from coal tar. I suppose you all know what madder roots are. Madder is the root of a plant termed the rubia tinctorum. It grows in Turkey, France, Russia, and various other countries, and is imported into England in large quantities for the sake of the beautiful and valuable dye which can be got from it. Everybody in Manchester, I suppose, knows what madder pinks and madder purples are. Now, what is it in the madder which gives these peculiar and beautiful colours? It is a red crystalline substance which has been prepared from madder, and to which the name of alizarine has been given; but we knew nothing of the mode of action of this colour until the year 1848, when Dr. Schunck, of Manchester, showed that all the finest madder colours contain this alizarine as their colouring principle. Dr. Schunck and Mr. Higgin next showed that this alizarine was not contained in the fresh madder root, but that the colour was only got when the substance of the madder root had undergone a peculiar kind of change—a sort of fermentation, in which a kind of madder-sugar or glucoside yielded, amongst other products, alizarine. And Dr. Schunck showed that it is to this alizarine that is to be ascribed the power which madder possesses of producing these distinct and beautiful tints which we know either as madder pinks or madder purples, as well as the brighter colour which we all know as Turkey red. Now the mode in which the colouring matter of madder, this alizarine, is brought on to cotton goods, is the point to which I wish to draw your attention. The colouring matter itself will not fasten on the cotton; it is not "fast;" that is to say, it will wash out; and therefore it is necessary, in order that we should get the colour fixed in the cloth, that it should be held down by something in the cloth, in a similar way to that in which the ammonia was held by the water. And this is done by what the dyers and calico printers term mordants. A mordant is a body

which enables the colouring matter to be fixed upon the cloth, to be laid hold of, as it were. And this is because the colouring matter forms with the mordant a solid substance, which is then fixed in the little pores and tubes of the cotton fibre. Thus the colour does not escape when the goods are washed, because it is held fast in the tubes as a coloured solid body, which is generally termed a "lake." These mordants are "printed" on the cloth in various patterns; where a red or pink colour is required, there the alumina mordant is impressed on the cloth; where a purple colour is needed there the iron mordant is printed, and this explains the fact that by dyeing the cloth thus prepared, in one dye beck with one colouring substance, madder, such different tints are obtained.

But now to get to our point with regard to the other example from the coal tar series of constructive chemistry. You will easily understand how desirable it would be to get these madder colours from the coal tar, for although not so beautiful and bright as the aniline colours, yet they possess properties which render them still more valuable; for we in this country prefer, as a rule, colours which are not so bright or glaring as the aniline colours; and, therefore, the reds and purples of madder will always be in large demand in this country as well as elsewhere. If now we could obtain from the coal oil this beautiful and valuable colour which is found in madder, the advantage would be of course very great. The truth of this will at once be evident when we learn that the total growth of madder in the world is estimated at 47,500 tons per annum, worth about £45 per ton, and having therefore a value of £2,150,000. Of this nearly one half is used in this country, so that no less than £1,000,000 is now paid each year by us for madder grown in foreign countries. Now two young German chemists, Messrs. Graebe & Liebermann, set to work to endeavour to perform this chemical synthesis; they began in a very workmanlike and a very scientific way; for instead of trying all the various bodies which are found in the coal tar to see which of them would yield this colouring matter, they began the other way about, and first took some of the natural colouring matter itself and tried to decompose it or split it up, in order that they might see what sort of a body this colouring matter would yield them; and they found that in reality this body when it was decomposed gave rise to a white substance, which, on analysis, they found to be identical in composition with one of these bodies which had been formerly found in coal tar, which had been named anthracene, a specimen of which you see

in the bottle. Here, then, was the first step; for they had proved that anthracene could be got from the colouring matter of the madder plant. Next, these two German chemists set themselves the opposite problem, which now had become much easier, inasmuch as they now knew the kind of skeleton, as it were, from which they had got to work to build up their wished-for structure; they set to work, I say, to endeavour by bringing together other compounds with this anthracene, to build up the colouring matter, of which, remember, they knew the composition, from the coal-tar product. And this they succeeded in doing. They actually obtained this beautiful red crystalline body from coal tar, which body possesses every property of that got from the madder plant, that essential which gives to madder its peculiar and its valuable qualities. Here, then, we have indeed a triumph of synthesis, and another proof, if one were needed, of the value of the results of constructive chemistry. This is the first case of a colouring matter contained in a plant having been artificially made. The beautiful colours derived from crude aniline do not exist in nature; they are altogether new, and are not found in any plant. But many other colours, besides alizarine, which are used largely in dyeing, occur only in plants.

Thus indigo is another well-known colour, but indigo has not yet been artificially prepared, though there is very little doubt that before long we shall be able to do so. Indigo is as yet only produced as the result of the life of a plant, and the artificial production of this valuable dye is a problem which yet remains to be solved.

Now this anthracene, although it is contained in comparatively small quantities in coal tar (100 tons of tar yielding only about half a ton of anthracene, or one ton of anthracene being got from the distillation of 2,000 tons of coal), yet still it can be got in absolutely large quantities, because such an enormous quantity of coal is distilled for gas making all the world over; and therefore if the processes of building up the alizarine from this anthracene be not too costly, there is little doubt that the artificial colour will be made in quantity, and a part at least of the money which we now send out of the country to buy madder roots will go to benefit our own population, as we can now transform our coal into this invaluable colouring matter.

Well, now, let me try to show you that the artificial alizarine which is got from coal tar possesses similar, or rather identical, colouring properties with the alizarine got from madder. It is impossible for me to enter into the minutiæ of the mode in which

anthracene can be converted into alizarine, for I should have to use formulæ, which I am afraid many of you would not under stand, and I must be content with referring those who wish for information on this subject to the annexed diagram, or to treatises on organic chemistry.

In the following Table we have a statement of the synthetic production of alizarine from its constituent elements.

SYNTHESIS OF ALIZARINE.

1. Acetylene by direct union of Carbon and Hydrogen in Electric Arc.

   $C_2 + H_2 = C_2 H_2$ (Berthelot, 1862.)

2. Benzol (Tri-acetylene) from Acetylene by Heat.

   $3\text{-}C_2 H_2 = C_6 H_6$ (Berthelot, 1866.)

3. Anthracene from Benzol and Ethylene.

   $2\, C_6 H_6 + C_2 H_4 = C_{14} H_{10} + 3\, H_2$ (Berthelot, 1866.)

4. Alizarine from Anthracene. (Process No. 1.)

   (Graebe and Liebermann, 1869.)

   (A) Oxyanthracene or Anthraquinone by Nitric Acid.

   $C_{14} H_6 (O H)_2$ (Anderson, 1861.)

   (B) Bibromanthraquinone by action of Bromine.

   $C_{14} H_8 O_2 + 2\, Br_2 = C_{14} H_6 Br_2 O_2 + 2\, Br H$

   (C) Alizarine by action of Caustic Potash.

   $C_{14} H_6 Br_2 O_2 + 4 K H O = C_{14} H_6 (O K)_2 O_2 + 2\, K Br + 2\, H_2 O$

   Potassium alizarate.

5. Alizarine from Anthracene. (Process No. 2.)

   (Graebe and Caro, Perkin, Schorlemmer and Dale.)

   (A) Disulphoanthraquinonic Acid from Anthraquinone.

   $$C_{14} H_6 (O H)_2 + 2\, H_2 S O_4 = C_{14} H_6 O_2 \left\{ \begin{matrix} S O_3 H \\ S O_3 H \end{matrix} \right\} + 2\, H_2 O$$

   (B) Alizarine from the above by the action of Potash.

   $$C_{14} H_6 O_2 \left\{ \begin{matrix} S O_3 H \\ S O_3 H \end{matrix} \right\} + 4 K H O = C_{14} H_6 O_2 \left\{ \begin{matrix} O H \\ O H \end{matrix} \right\} + 2 K_2 S O_3 + 2 H_2 O$$

   Alizarine.

CONTRIBUTIONS TO THE HISTORY OF ALIZARINE. $C_{14} H_8 O_4$

1825. Faraday discovered Benzol in Coal-gas Oil. $C_6 H_6$
1831. Robiquet and Colin discovered Alizarine in Madder Root
1832. Dumas and Laurent discovered Anthracene in Coal Oils
1848. Schunck gave the Composition of Alizarine. $C_{14} H_{10} O_4$
1850. Strecker ,, ,, ,, $C_{10} H_6 O_3$;
1862. Anderson examined Anthracene Compounds. $C_{14} H_{10}$
1865. Kekulé explained the constitution of the Aromatic Compounds
1866. Baeyer obtained Benzol from Phenol
1868. Graebe investigated the Quinones.
1868. Graebe and Liebermann obtained Anthracene from Alizarine.
1869. ,, ,, ,, Alizarine from Anthracene

The point, however, which all of you can understand is that we are now using this method of constructive chemistry for the purpose of building up substances which up to this time have only been found in the bodies of plants or animals.

One of the most remarkable properties of the alizarine got from madder is its power of forming an insoluble compound with a mordant. I have here the alumina mordant, or red liquor, which forms, with alizarine, a pink insoluble lake; and here I have the iron liquor, or iron mordant, a solution of a salt of iron, which forms, with alizarine a purple insoluble lake. I pour some of these mordants into both these bottles of water; next I bring into one some extract of madder root, some of the natural alizarine got from the plant. You will observe we get here a bright red precipitate. Next I take the artificial alizarine made from coal tar, and I pour this into the other globe of water to which I added some alumina mordant. You will see that I get exactly the same sort of red coloured precipitate. One is the natural, the other the artificial, and both give exactly the same kind of colour. In the same way, if I take and compare the effect of the iron mordant, I shall find that both the natural and the artificial colour give exactly the same purple precipitate.

Now in order to show you in another way the identity of these two things, we have written here on this screen the words "natural alizarine" and "artificial alizarine," and when these are sponged at the back with alkali you will see that we get the same colour exactly produced by the two kinds of alizarine. By burning a bit of magnesium wire the purple colour of the alkaline alizarine will be better seen, and you will observe that we have got exactly the same tint in both cases. I will show you the same thing by dyeing some cloth with the artificial and with the natural alizarine. Here we throw a very small quantity of the madder alizarine into a basin-full of boiling water, and here do the same with the artificial colouring matter, then I bring into each basin a little bit of mordanted cloth. I won't say that we can get a very fine colour, but you will see that the colour we get is equal in the two cases, that the artificial alizarine produces the same colour as the natural. We will allow these cloths to remain a little while in the boiling liquor, and now on taking them out you see that the alumina pinks are in both cases equally bright and the iron purples also exactly of the same shade and tint. Thus, then, we see that the artificial alizarine is exactly identical in its dyeing and colouring power with the colouring matter contained in and derived from the madder root. How far the artificial alizarine

will in time displace the madder it is not for me to say; this is a question which I will leave to the calico printers and dyers of this great district; but certain it is, that the two are chemically the same substance, and that this production of alizarine from coal tar is one of the greatest triumphs of modern synthetical chemistry. This new dyeing substance is now being largely used on all hands, especially for what is called topical printing and for Turkey red dyeing, and I am told that the colours which can be obtained from the artificial alizarine are quite equal, if not superior, to those which can be obtained from the natural madder.

And now if we are to draw a moral from all this, I think that we shall have little difficulty in doing so. These facts show us the truth of the old saying that great results come from small beginnings; they teach us that nothing in science is unimportant; that no one can foresee the benefits which to-morrow may spring from our apparently abstruse discoveries of to-day. Science is advancing, and its progress, unlike that of so many human institutions, is without the possibility of retrogression. Boldly, then, may the least of its votaries step forward, in the firm conviction that the degree, however insignificant, by which he may be able to advance the boundaries of science is a certain progress, and one which must add its share towards the enlightenment and benefit of mankind.

THE ORIGIN OF THE ENGLISH PEOPLE.

A LECTURE

BY PROFESSOR A. S. WILKINS, M.A.

Delivered in the Hulme Town Hall, Manchester, November 16th, 1871.

I HAVE undertaken to speak to you this evening on a branch of science which I think has not before been brought under your notice. This course of lectures has hitherto been confined to those branches of science which deal especially with the things which we see around us. To-night I am going to confine your attention almost entirely to things which you *hear* round about you. And I want to discuss these things that you hear—the words that we are using in daily life—somewhat after the manner in which other scientific men deal with things which we see, the objects of sight. You know that chemists such as Dr. Roscoe, and the distinguished chemist whom we are to have next Friday evening, Dr. Odling, make it their business to examine into everything which they can find in the heavens above, in the earth beneath, and in the waters under the earth. They will tell you what these things are composed of; they will split them up, analyse them, as they call it, into their remotest and most ultimate constituents. Now, the geologists, on the other hand, may be said not to trouble themselves quite so much with the composition of the substances they deal with; but they are concerned perhaps more with the manner in which they got into their present position.

I want to try this evening to show you, as far as I may be able in the short time during which I can hope to have your attention—

for the lecture is necessarily not illustrated by any experiments—both how those words which we are using are made up; and also how they came to be in their present position. I have said that I am not able to show you anything to see. I had hoped that I should have had a map which would have enabled me to explain at least some of the facts which I wish to bring before you a little more clearly than I shall now be able to do; but in this I have been disappointed, so I must, I suppose, ask for your special indulgence, on the ground that you will have to listen and not to see during almost the whole of the time allotted to us.

Now if we begin to split up, or to analyse, or to examine closely, the words which we are using in daily life, we shall find that a fair proportion of them, quite a considerable proportion, are very closely akin to the words which Welshmen would use. I do not mean to say that we use them in exactly the same form in which Welshmen would use them; but at all events the words are very strikingly like Welsh words. This is the case with the English that is spoken all over this country of ours. For instance, when you want to speak of an article of dress, you may talk about a *coat*; you may talk about a *gown*; you may talk about *frieze*, from which you would make the coat; and to come to smaller points, you may talk about a *button*, a *tassel*, of the *gussets* in shirts, of *welts* on shoes, and of *clouts* and *dishclouts*. In all these cases we are using words which are almost exactly like words which Welshmen would use in such cases. If we come to our household things, if we talk about a *basket*, a *barrow*, a *funnel*, a *pitcher*, or if we talk about *crockery*—in all these cases we are still using the same class of words. And here in Lancashire we use a good many of these Welsh-like words, which scientific scholars call Keltic words, which are not known or understood in the rest of England. If I were talking to people in the south, I dare say they would not understand what I mean by *bamming*. You may know, perhaps. So in the same way they do not know what *boggarts* are. They would not understand what I meant if I talked about a man being a *farrant* or a *gradely* man; if we talked about setting *craddies*; if we talked about *cobbing*, or *wapping*, or *punsing*—all these words would be unknown in the south; and I think I may suppose they are pretty well known here. If we hear that a man is a cunning *file*, it has nothing whatever to do with the file that a blacksmith would use. That again is only another form of a Welsh word, meaning a *twisty* fellow. In the same way, if you talk about going out for a *spree*, and of playing fine *pranks*, in all these instances you are talking Welsh or Keltic words. The same

thing would be true, if in your business you talked about a cotton *gin*, or a weaver spoke about his *picking stick*. Here again we still keep to the Keltic element of our language.

Now, one of the first questions that men ask who wish to go into a subject of this kind scientifically is—How did these words get into our language? Of course there are several ways in which words not belonging to a language originally may come into it. We may borrow them. For instance, we use the word *gutta percha* to describe a substance well known to all of us. That is not an old English word, we get the name from the country where we get the thing from. Just in the same way with *coffee;* where we get the coffee berry we also get its name. There is another way in which words may be borrowed, that is, from fashion. For instance, we have borrowed a great many French words, and many people now-a-days very foolishly, I think we may say, prefer to use French words where good English words would do as well. Nobody, I suppose, imagines that *coats* were never known in England until Welshmen came here and brought them, or *gowns* or *buttons*; that *cobbing* or *wapping* was unknown until Welshmen taught it us. We must try to find some other method of explaining the presence of these words in our language. That is one of the questions that we shall have to try to answer to-night.

But now, when we go on and try to analyse or to account for other words in the language that we are talking about, we find a good many of them come from the Latin. Some of them come straight away, very little changed in their passage, so that the man who knows Latin, whatever country he belongs to, would be able to understand this sort of English words. A good number of them are words that everybody knows now, words like *science*, or *student*, or *origin*, or *admit*, or *adopt*—plenty of words of that kind which have become part and parcel of our everyday English talk. And there are a great number of other Latin words which are used perhaps solely in sermons or solely in scientific treatises, which are not known to us usually in everyday talk, but which we have to learn specially, and which have come directly from the Latin to us. But besides this kind of Latin words, we have another set of words which scholars are able to derive from the Latin, but not directly; they have got so much changed on their way, that they seem to have gone through a different kind of process, have been sifted or moulded in some way, generally cut shorter at the head or the tail, or at both. Such words, for instance, as *cover*, or *obtain*, *complain*, *hour*, *flower*—words of that

kind are abundant, and certainly they are not old-fashioned English words; they are the children, perhaps in this case I ought rather to say the grandchildren, of Latin words; but they have taken such a changed form in their passage from Latin into English, that we cannot suppose they were borrowed straight from the one language for the use of the other. When we examine these words further we find that they are not exactly like Latin words, but they are almost exactly like French words. I can give you some instances of words which we have got straight from the Latin, and words which originally come from Latin have come to us through French. For instance, we may talk about food being *nutritious*, or we may talk about food being *nourishing*. These words have precisely the same origin, and have precisely the same meaning; but one of them has come to us through the French, and so it has got a little bit changed on its way. In the same way, to give you a more striking instance of the same kind, we have the word *preach*. We have another word which has come directly from the Latin, not through the French, and therefore is longer and fuller,—a word which is not commonly used, but may be found sometimes in the leading articles in newspapers, and other writings of that kind—the word *predicate*. These words are the same in origin, but have got a good deal changed one from the other. So, again, the *poor* man is not always a *pauper*, but the word *poor* is only a shortened form of the word *pauper*, that has come to us through the French. *Story* is not quite the same thing now-a-days as *history*, and the shortening is to be explained in the same way. So a *mayor*, the chief magistrate of a borough, is a different person from a *major* now-a-days, but originally they were the same. So, to give a more striking instance—one which might not have struck you at first when you saw it—the word *spice*, which we now apply to fragrant things like nutmeg and pepper, &c., is exactly the same word as the word *species*—of which we have heard a great deal lately—modified both in form and in meaning on its way to us.

Well, now, you see we have two more questions to solve, if we can. Not only are there these Keltic or Welsh-like words in our language, but there are Latin words very little changed, and Latin words a great deal changed—so that they are very much more like French words than Latin words.

You may naturally ask here what proportion of words in our language can thus be traced back to the Latin. That depends to a certain extent upon the way in which you count words. Suppose you put all the different words you find in any writer into a

dictionary or an index, not repeating the same word more than once, you will find perhaps one word in four Latin. The proportion varies very much, the simpler and plainer and the more straightforward the style of the writer, the fewer of these Latinised words he will use; the more involved and pompous and formal and generally unintelligible his writing is, the more of these Latin words he will use: so that in our old English Bible—which is among other things just the very finest specimen of the English language that we have—sometimes out of a hundred words you will only find four that are not good plain English; and in the hardest places, where Latin words seem almost necessary, you will not find more than ten in a hundred. Shakspere, too, who usually says what he means in a way which most of us can understand easily, will only use perhaps from nine to a dozen out of a hundred words. Milton, who was more stately and formal in his style than Shakspere, will use generally about twenty. Dr. Johnson twenty-five, and the great historian who wrote about a hundred years ago, Gibbon, will use sometimes thirty. But this is when you arrange the words in a sort of index, counting each word only once. But suppose, on the other hand, you take a piece of English just as it is written, then plain, simple English words will come over a good deal oftener than that. To get a fair specimen of the English that is talked now-a-days, when a man wishes to make his meaning as plain as he can, I took a speech which was delivered a little while ago by the Bishop of this diocese. You know that he always tries to make himself understood as plainly as he can; and out of some three hundred words that he used, I find there are about fifty belonging to this class which we are now discussing. What are we to say of the rest? Well, of course, we have here and there a word got from almost every language under heaven; because, generally, wherever we have got anything new, there we get the name for it; but almost the whole of the rest of our language, that is to say, perhaps two words out of every three, belong to what is called the German class of languages—not quite the German that is spoken now-a-days by the educated people in Germany, for our language is based upon what is called the Low German. No disrespect is intended to it by that phrase; it simply means the sort of German that is talked in the low region near the sea, and not in the more hilly region inland. The High German, as it is called, differs from the Low German in several ways, some of which it would take me perhaps too long to explain now; but I think I can give you with very little trouble an idea of one of the main differences between the Low Ger

man on which our language is based, and which our English really is, and the High German which Germans now-a-days speak. Suppose you pronounce any vowel sound, say *a*; as long as you pronounce that vowel sound you are letting one uninterrupted stream of breath come out of your lungs, play on a little instrument at the top of your throat which determines the sound you produce, and then pass into the air unchecked. So if you simply content yourself with pronouncing a vowel, you can go on as long as you please with it—*ā-ā-ā*—as long as you have breath. But you can check that stream of air, producing sound, in three different ways. You may check it in your throat, and then let it go on again, and then you will pronounce a consonant like *k*. Or you may check it at the top of your tongue, and then you will pronounce the consonant *t*. Or you may check it with your lips and then you will pronounce the consonant *p*. You can say *kay*, *tay*, *pay*. But then checking it in just the same place you can produce sounds that are a little different from those. I can say in my throat not only *kay* but also *gay*; not only *pay* but also *bay*. Well, those who are concerned with the scientific examination of sounds have given names to these different letters. Those which I gave at first they call properly *surds*; those which I gave in the second instance they call *sonants*; for this reason, when you pronounce *b* or *g* or *d* you make a vocal sound in your throat at the actual time you are pronouncing that letter; but when you say *p*, *t*, or *k*, you do not. Now it is a little more trouble to pronounce those which make a sound in your throat, which we call *sonants*, than those which do not produce a sound in your throat, which we call *surds*. You can easily test that for yourselves. It is a little more trouble to say *bad* than it is to say *pat*, and the people who talk the High German language have got into this lazier or more slovenly way of pronouncing, using the surd instead of the sonant letters. And you will find that that is really the main difference between the High German the Germans talk and the Low German that we English still talk. For instance, when we talk about a *dale* they will talk about a *tal*; if we say *door* they will say *tor*; if we talk about *daughter* they will say *tochter*, if we say *drink* they will say *trink*, and so on. Then further, when we get the *t* sounds they will soften them down still more into *th* or *z*, not completely cutting off the stream. For instance, our *ten* is their *zehn*; our *tongue* is their *zunge*; our *tear* is their *zerren*. When the *t*, instead of beginning a word, comes in the middle or at the end, they make a further change. You know now-a-days instead of saying he hath, or he loveth, we

generally say he has, or he loves. The Germans have adopted just the same change, changing our *t's* into *s's;* so that when we say *white* they will say *weiss;* for *water* they will say *wasser*, and so on. But with these exceptions, we are talking in the basis of our language, that is to say, in simple, every-day words, mainly the same sort of language as our German cousins.

Now we have to consider how to explain these facts. We have got a fourth one now in addition to our three problems before. How is it we use Welsh words? How is it we use Latin words? How is it we use Latin words that seem to have come to us through the French? And how is it, finally, that the basis of our language is just the same as the German which is spoken on the coast of Germany? History has to help us to explain these facts. If we go back as far as ever we can in the history of man—I do not mean as far as Mr. Darwin would take us back, but as far as we can go back with the men with whom we have any sort of concern as our fellow men—we find that there must have been some great hive somewhere about the middle of Western Asia, which was constantly sending forth swarms of people, for the most part always westward. Then when one swarm—if I may use the language they would use of bees—had come out, they would settle down in some territory which they liked, until another swarm came from behind, and finding this territory suited them also, they would drive those who had gone before them a little further to the west; and so on, until we are able to trace at least five distinct waves of people coming one after the other from this part of Asia that I speak of—very much that same part where the Bible tells us Noah landed out of his ark—and always pushing before them those who had gone first. Now you know that those who live furthest to the west of all the people of Europe are the people of Ireland; therefore we think we are justified in assuming that the Irish were probably the first to leave, and then they got pushed further and further on towards the west always, till they got pushed so far that they could not go any farther without being pushed into the sea. Then, of course, they had not discovered the way to America; now they are pushed right beyond the sea into America. We know this principally because we find them at the extreme west. We know they could not have come over the water from America; we know that they did not grow as a nation where they are now; therefore they must have come the other way. We have additional proof of this in the fact that all about the continent of Europe there are names which we can show to

be properly Irish names. I shall come back to this question if I have time this evening—this question of the meaning of local names. The Irish have left very few traces of their passage through England; but I think, we may find one or two traces of the time when England was peopled principally by those who are now living in Ireland, but they are not at all certain, and I should not like to give them to you as facts. But we do know that there are plenty of traces of the next great wave, and those are the people who are now the Welsh. They live the next towards the west. The people at the top of Scotland were probably originally the same as the people of Wales. We judge of that also by the evidence of local names; the names of places. About 1,400 or 1,500 years ago, some tribes of Irishmen who called themselves Scots—because you must remember that the Scotch came first from Ireland—came back into Scotland, and practically absorbed or exterminated the Welsh folk who lived in Scotland then, and took the country for themselves; so that now-a-days the people in the north of Scotland, the Highlands, and the people in Ireland speak languages which are very closely akin to each other, but not so closely akin to the Welsh as the language of the Highlands used to be. Then, just about 1,800 years ago, the Romans came—they had been here a hundred years before that, but their expedition failed—and they conquered all those Welshmen, or Kelts, as we call them sometimes, who dwelt in England and Wales—it was not England then, it was Britain—and subdued them entirely under their dominion. They remained about 400 years, and then they withdrew. And before they had gone long, swarms of these Low Germans came over. I use the word Low, you must remember, always in its technical sense, meaning the Germans living by the sea coast, not in the way of disparagement. They lived in that part of Germany which is just at the bottom of Denmark, where Denmark joins on to the main land, just about Schleswig Holstein, of which we heard so much six or eight years ago. They came over in their families and tribes, as I shall be able to show you by this same evidence of names of places, and conquered England by degrees. There were two tribes; one called themselves Saxons, and the other called themselves Angles, from which we get our name of England. They did not come over all together; they kept coming over for nearly a hundred years, one swarm after another, moving with their wives and their children, and perhaps their cattle also, and settling here, driving the old Welsh people, who lived all about the country then, before them, till they cooped them up into the western parts, *i.e.*, Cornwall,

Wales, Cumberland, and Westmorland. They left a good many of them in Lancashire. To speak very roughly, if you draw a line from Chester to London, you will find that the Saxons lived to the south-west of this line, and the Angles, or the English, lived in the north-eastern part, right away up as far as Edinburgh. I will show you one means by which you can tell that at once. Look at those places which end in *sex;* Sussex, South Saxons lived there; Essex, East Saxons lived there; Middlesex, the Middle Saxons lived there. And in the old days, before these counties were so split up, all this part was called Wessex, that is to say, where the West Saxons lived. On the other hand, as you may know still from the name of one of our railways, all this part was called East Anglia, and by degrees the name Anglia in Latin, or in English Angle Land, spread over the country.

There is a subject which has been much discussed by scholars as to how it was that we came to be called English and not Saxons. If you are going about in Wales and you meet one of the rough peasantry and you ask him the way to any place, the answer you will probably get will be *Dim Sassenach*—I know no English; in other fashion, I know no Saxon—another proof, as I have shown you, that the people with whom the Welsh came into contact were the Saxon people.

Two theories have been started to explain this; there may be something in both of them. In the first place there were a good many more Angles than there were Saxons. In the second place those people who first came into contact with the missionaries who came over from Rome to convert the German invaders to Christianity (for when they came over they were pagans) were the Angles, and so the missionaries called the whole people Angles, and the name came to be gradually accepted; it got used in books, and then by degrees it was used generally. The Angles and Saxons founded several small kingdoms: one of them, the kingdom of Northumberland, stretched to the south and west beyond Manchester; and in an old book I have read of Manchester in Northumberland, not because they thought it was up there, but because in that time Northumberland stretched from here right away to Edinburgh. And just about the time when these various kingdoms were first brought under one king, other swarms, very much resembling those Saxons and Angles which had first come over, came from Denmark and Norway; and they pillaged the coasts when they came in small numbers, and when they came in large numbers they formed armies which conquered large portions of the country for themselves; so that after nearly

a hundred years' hard fighting between them and the English people they succeeded in getting a firm footing on the ground. And almost the same part of the country which I said was held by the Angles was given up to the Danes, under the name of the Danelagh. At the same time the Norwegians came sailing round Scotland and conquered the Isle of Man, and settled in large numbers in Cumberland and Westmorland and North Lancashire, and all along this part of the coast, in fact; and I shall be able in a minute or two, I hope, to show you what tokens we have still of their presence.

Our English kings—the old English race of kings—reigned for nearly 300 years after England had been made a united monarchy, and then the last of them, Edward the Confessor, died without leaving any children. The English people in those days had the right of choosing their kings freely. They always exercised it by choosing one of the royal family, but they chose not always the eldest son, but the man whom they thought fittest to rule, the bravest, the wisest, and strongest. But now all the old English royal family was extinct, except one distant relation, who was a mere boy, and whom the English people did not think worthy to rule over them. So they chose a great earl of the time, Earl Harold, whose father had been the son of a swineherd, and had raised himself by his valour and ability to the rank of the first man in the kingdom. But there was some sort of claim upon the crown—not a very good one—on the part of the Duke of Normandy, and he put forth his claim. He said that as there was no nearer heir to the crown, it fell by right to him. The English people held firmly to the king they had chosen; but William, the Duke of Normandy, gathered a large body of French troops, and came over, and, as most of you know, defeated the English king, Harold, at the great battle of Hastings, and killed him, and succeeded in compelling the English to choose him as their king. This is what is meant by the Norman Conquest. The word has often been misunderstood; it is not very happily chosen perhaps, because it was not that the English people were conquered by a foreign people, but rather that the foreign king was strong enough to make the English people choose him as their king. However, the result was at first sight very injurious to the English language and laws, because the foreign king was surrounded by a large body of French nobles and captains, to whom were given large estates, and French and not English was made the prevailing language for something like two centuries. This Duke of Normandy had also large possessions in France, and the first six of

these Norman kings were much more Frenchmen than Englishmen. We read in our history books about Richard the Lion Hearted, and think him a fine specimen of an English king, but it is extremely doubtful whether he could ever speak a word of English in his life; and it is very certain that he only spent two or three months in England, and that was when he came over here to get money out of the people. However, his brother, the bad John, lost all his dominions in France, and was driven out of them by the French king, and so England became again an independent kingdom, without any possessions other than those within her own boundaries. The result of this was that there was no longer any occasion for French to be the language of the court and of the nobles. It continued to be so for a short time, because they were accustomed to speak it; but it was not very long before the English language raised its head again. It had never been disused; it had always held its own among the common people. Their songs were written in English—we have many of them remaining to us—and they had always talked it among themselves, but it had been looked down upon. Now that the English noblemen were shut out from their foreign possessions they began to be proud of the name of Englishmen, and they began to learn by degrees to talk the English language. But they mixed it up with a great many of the French words which they had been accustomed to use. And now I think you will be able to see how it is that we have got these four elements in our language which I was speaking about. I do not know whether you noticed when I was talking about the Keltic words, that they were either words relating to home affairs, or else familiar and somewhat vulgar phrases. A large number of the coarse and bad words that we use now-a-days are Keltic words. That points to the fact, which you would naturally expect, that when the Saxons and English people who came over (after the Romans had left this country) and conquered the Welsh people, those whom they left in the land they made their slaves; and so they would naturally get from them just those words which were necessary to explain to their slaves what they wanted. The words which I named before, like coat, or gown, or basket, or barrow, are the words which would be common among the household slaves, and they would be used by the Keltic or Welsh slaves who were made so by the Anglo-Saxons. You see also how it is we have so many German words, because these people, when they came from North Germany and crossed over to conquer England (Britain as it then was), would naturally bring their own language with them.

The French words came in from the Norman Conquest; and though it is not true to speak of English as a mixture of this Low German and French, yet it has borrowed a good many French words which are incorporated with its own, and are made one with its own substance. And then the Latin words are to be explained from this fact, that for many hundred years Latin was the only language that was written and used by learned men in all the countries of Europe; and whenever they wanted a word for something which they did not know how to express in the plain English of the common folk, they would borrow it from the Latin with which they were familiar. That is the way in which we explain the four elements which we get in our language.

Now I want to show you another side of this question, and that is, the light which the names of places throw upon the origin of the English people. The first population of this country, you will remember (supposing we put aside for a moment the possibility, or I should rather say the probability, that the Irish people lived here before they were driven across to their own country), was the Welsh division of the Keltic stock. Now the first places which would require names, of course, would be the rivers and mountains. When the Welsh came to the country they would want a name of course for a river, and a name for a mountain, for there were no towns as yet; and so we find that almost all the names of rivers and mountains in England are nearly Keltic. Take for instance a few of the Keltic words that we find in proper names. One of the Welsh words now-a-days for a river is *avon*. Well, however little you know about the rivers of our English country, you must remember several of them that are called Avon. There is the Avon on which is Stratford, Shakspere's birthplace; there is the Avon in Somersetshire, where Bristol is; and there are several others. This word avon simply means river, and we call the river by Bristol Avon simply because the Welshmen who lived in our country 2,000 or 2,500 years ago called the river by a name which in their language meant river. There is another word, *dwr*, which means water. We get that in plenty of our words. In the Lake country we have the Derwent and Derwentwater. Derwent simply means *clear water*. In the same way that other beautiful lake is named Windermere, which is simply *beautiful water*. *Wyn* is beautiful, *dwr* is water in the language of old Welsh, and *mere*,—you know that from Rostherne Mere, and so on. We get the same in the names of many rivers. You know the Calder here, it flows along by Todmorden; that is again a crooked or winding water. And wherever we have a word with a

meaning of this kind in Welsh, we may be quite sure that it was Welsh people who gave it that name. Therefore, if we find a river called the Calder, we may be quite sure that the first people who came to that river were Welshmen. There is another name which has got a good deal changed, but perhaps it is the most widely-spread of all, and that is *Wysg*—which also means "water." If I should have any Irish people here to-night, they will pretty well understand, I think, what is meant by usquebagh; that has the same root—water. Well, this occurs in many of the names of rivers in England, only a little modified. There are two or three rivers called Ouse; other rivers called Exe, Axe, Esk, or Usk. All these names of rivers simply show that Welsh or Keltic people came there, and when they found a stream of water they called it in their language river, or water. The Ribble, which flows by Preston, is again another Welsh word, which means simply "fast river." Then the same word Avon, which I spoke to you about before, comes in in a good many compound names. Take, for instance, this county in which we are in now. It is called Lancashire because it is the shire of Lancaster. I will talk about the second part of it afterwards. Lancaster is called so because it is on the Lune, which, in old days, used to be called Alauna. Words always have a tendency to grow shorter the longer they live. A distinguished English scholar said once that letters were like soldiers, they had a great tendency to drop off on a long march. And I could find dozens, hundreds, thousands, literally, of instances in our English language in which words have got shorter. To give you just one example. Our word "ma'am," which some persons would use in addressing a lady, is cut short from a phrase which originally had five syllables at least. So the name of the Lune was Alauna, and that in the language of the Welsh people simply means "white water." So we call the county town Lancaster—that is, the camp or castle that is on the white water river. Then there is the opposite word in Welsh, *dhu*, which means black. Thus we get Douglas, or in the shorter form, Diggles, meaning "black water." There is a word which you have still in Lancashire, *cam*, which means crooked. It is a word that Shakespere uses. We get that in several forms, Camden, for instance. Another instance which most of you remember is Morecambe Bay, that is, the crooked sea. You remember how the sea goes in and out there, and Morecambe must have been called the crooked sea at the time when Welsh people lived there, to whom this word Morecambe would mean crooked sea. If time would allow me, I could show you in the

same way that Irwell (the quick, winding stream), Irk (the leaper), Med-lock (the full pool), all preserve in their names signs that the Welsh were here before us. But to pass on from rivers to hills, we have *pen* the Welsh word for hill; which of course we get in Pendleton, which is simply hill town; Pendlebury, another form of the same name; and the hill which is above Clitheroe, Pendle Hill. In Wales and Cornwall it is a very common name—Penrhyn, Penmaenmaur, Pendennis: in all cases *pen* meaning hill. And wherever we find this word *pen* it means simply that the Welshman was there before us and talked about the "hill." Coniston Old Man is called so simply from the Welsh Alt Maen (high mountain), and has nothing to do with any old gentleman.

Of town names we have very few that are Keltic, for the natural reason that the Welsh folk who lived here in Lancashire once had very few towns to give any names to. In Doomsday Book, which gives us a very complete account of the country a few years after the Normans came here, I find that only 16 villages are mentioned as existing then in the whole of Lancashire. So that it need not surprise us if we find that Wigan is about the only instance of a Keltic name for a town: this means "battles," and the place is so called because of some battles that were fought there in very early times.

Now, let us pass on. We have seen that the Kelts were here; the Romans came after them. They have left us very few names. One or two will be of interest here. Their word for camp was *castra*, which we get in Lancaster. We know that Lancaster must have been at least as old as the Roman times, because no other people but Romans would have talked about "castra" for camp, therefore it must have been Romans who gave the name of Lancaster to the city or town which was built on the river which before then the Welsh people had called the Lune or the Alauna—the "white water." So with the name of this city, Manchester. "Chester" is only the softened form of this same "castra." In all languages that I know anything about there are instances of this changing of sounds. The *k* sound gets softened by degrees either into *s* or *ts* or *ch*. So Manchester means a camp or fortified place. But what does the "man" mean? If you believe that the Welsh word *man* means a plain, and if you will just ride from Cheetham Hill down to here, you will, I think, easily see why Manchester was called "the camp at the edge of the plain." If you go to the north of Manchester, you get into the hill country at once; if you go south—as those know who live on this side, you get very little hill, but just a broad, flat plain.

Manchester means a camp, or a fortified place which was built by these Romans, just at the place where the great flat plain of South Lancashire and Cheshire begins.

We have only one other instance perhaps worth troubling you about, and that because of its local interest. We have another Roman word remaining to us, in *street*. "Street" is an old Roman word for road. Some of you may know High Street, in Westmorland, the high mountain over which the Roman road runs at the top; and an old Roman road runs down to Stretford, that is, where the "street" went over the river. Camp Field is a later name; it has nothing to do with the Romans; here we get the English again. Now we have plenty of local names which are English. And here is one thing to be noticed at once—we do not talk now-a-days about Avon, but rather the River Avon, the River Usk, and so on. That points us to this fact, that when the English people came here, if they saw a river they asked what it was called. The Welsh people would say "avon," that is "river," Now the English did not know that avon meant river; they thought that was the proper name of it, just as we say Irwell, or Irk; and they would put their word "river" on to this word, whatever it might be—Ouse, or Avon, or so on. So we get River Ouse, River Avon. In just the same way we get Pendle Hill. The English people on coming would ask what that hill was called. The people there would say it was *pen*. Then the English coming would call it Pen Hill, and that would soon get changed into Pendle, and the hill which is near Clitheroe is still often called Pendle, and when hill gets mixed up with pen, the people forget that there is the word hill in the name; and so they put another hill, and talk of Pendle Hill, which simply means Hill, Hill, Hill! Just the same with Pendleton; that is Hill, Hill Town; Pendlebury, Hill, Hill Borough. We have a curious instance of this, which may have escaped many of you, here in Cornbrook. Brook is intelligible enough, but what is the "corn?" Of course, we suppose at first sight that it is a brook that ran through cornfields; it must have been a long time ago if it did! But we should be going quite wrong if we judged so hastily. Corn is simply our old word avon cut short, with the Welsh prefix *cor*, which means narrow. Now there is the Irwell, a comparatively broad stream, and the *cor-an*, narrow stream flowing into it The old Welsh people called it the *Corn*, that is, the narrow stream. The people coming afterwards asked what stream that was, and were told the Corn, or narrow stream. The English put on "brook," and so we get Cornbrook, narrow stream brook

We can tell very well wherever the English people proper have been by the terminations. There is an old rhyme that runs—

> **In Ford, in Ham, in Ley, in Ton,**
> **The most of English surnames run.**

And whenever we find any words with these endings, you may be sure that there the English people settled, not Welsh people, not Danish people, not French people, but simply the English, either Angles or Saxons. Wherever we have a word ending in *ton*, as we have abundantly here, Pendleton, Bolton, Middleton; wherever we have them ending in *ley*. as in Alderley and Timperley, and so many places in Cheshire; wherever we have *ham*, and in most cases where we have *ford**—in these instances you may be sure that the words are of English origin. I am not sure whether I shall have time to explain all these terminations. *Ton* simply means a sort of enclosure, more like a farmyard than a town. We have Barton-on-Irwell. *Bar*, the first part of it, is simply *bear*, and *ton* is the enclosure; and so Barton means the enclosure for what was borne by the ground, that is to say, for the harvest or the crop. Barton means a sort of farm yard or rick yard. That accounts for the fact that we have so many Bartons all over England, because there are so many enclosures where people put up their harvest produce. In "Broughton," near here, we have the same ending; and if any of you had the misfortune to live in Lower Broughton during the floods, you will understand why it was called Broughton, when I tell you that the first part of it means marshy ground.

In one name that we have near here, we get an instance of what is extremely important and interesting in its way—that is, Withington. Now here we have not so many of them, but in some parts of England there are a great many names ending in this *ington*. We have a fair number of them about here. You know we have Bollington, Carrington, Doddington, Rivington, Warrington. And then we have some in *ham*—Altringham, Aldingham, and Birmingham. And besides these, we have some words which end simply in *ing*—Melling, Pilling, and Billing, all just about this part of Lancashire. But as I have said, there are nothing like so many in Lancashire as in some other parts of England. In all Lancashire we have only 19 names with this *ing* in them, but in the little county of Bedfordshire we have 63; in Huntingdon-

* Fords by the sea are of Danish origin, and contain their word *fiord*, our *frith*.

shire we have 57; and in Kent 51 names having this *ing* in them. Well, of course, just as the chemist as soon as he gets hold of any substance whatever, no matter whether animal, vegetable, or mineral, wants to find out what its composition is, so we want to find out what this *ing* means. And we go back as far as we can, and we find that our old English forefathers used this termination *ing* to denote the son of a person. Suppose a man was named Eoppa, his son would be named Eopping, and all his sons would be named Eoppings. Suppose it was Boll, his family would be named Bollings. For instance, in our oldest version of the list of fathers and sons at the beginning of our New Testament, we have just the same form used; they would put *ing* on to the name of the father to denote the son. Wherever we have this *ing* we have an intimation and a proof, we may say, that the people who founded the town were all of one family, one little tribe, the children of a man called *Boll*, or something of the kind. Warrington is the *ton*, the enclosure, the village, we may say, of the children of *Wara*; and that is a proof of the fact which I told you on other authorities, that when our English forefathers came over from Germany, they did not come separately, like the Danes, but they came in families, altogether, "clans," as the Scotchmen call them. *Ing* means just the same thing as the Scotch "Mac," or the Irish "O'."

The Danes, I told you, lived in this part (north and east), and the Saxons in this (south and west). I will just mention the fact, though I cannot bring out the full meaning of it now, that here (north) you will find lots of *bys*, and in this part (south) lots of *tons*. Wherever you find places ending in *by*, as Whitby, Derby, Rugby, there you find Danes have been. *By* is the old Danish form for town or borough; and when you talk about "by laws" you simply mean the borough laws as distinguished from the laws of the country. Of course now we use the phrase for the laws of a railway or a club; but originally *by-law* meant borough law, as distinguishing it from the national law of the great Parliament. Here you find lots of *bys*, and here lived the Danes; here you will find *tons*, and English folk settled there. In Lancashire you will find *bys*, as Crosby, Formby, in the West Derby Hundred, and so on; that means that the Danes, sailing round the country with their ships, came and settled just on the sea coast, but could not get any further inland, because the English people drove them away. Hence you find them chiefly on the coast.

I meant to tell you much more about these Danish settlements, and also about the manner in which local names bear witness to

the presence of Norwegians rather than Danes in Cumberland and Westmorland. I should like also to show you how we know from names where the Angles settled and where the Saxons, but I cannot allow myself to try your very great patience any longer. I will simply assure you I have only given you this evening a very slight sample of the interest you may find in the scientific study of language.

THE FOOD OF PLANTS.

A LECTURE

BY PROFESSOR ODLING, F.R.S.,

Delivered in the Hulme Town Hall, Manchester, 24th November, 1871.

You all know that a piece of wood, or any quantity of wood, when set fire to, is capable of being burned entirely away, with the exception of a small—almost insignificant—residue of white ash which is left. [Holding up a piece of burning wood.] This white ash is spoken of as the mineral matter of the wood, from the circumstance of its being of the same nature as the matter of which our most common rocks and minerals are composed; whereas that portion of the wood which burns away is called the organic matter of the wood, from its being the matter of which the living, growing plant, with its different parts or organs, is mainly constituted. Now, when a piece of wood is exposed to the action of heat—by being thrust into the fire, for example—it gives off gases, and these gases, taking fire, burn with flame. A short time back Professor Roscoe showed you that when coal was heated in the bowl of a tobacco pipe, it gave off inflammable gases which might be burnt at the other end of the pipe; and, in the same manner that the coal when heated gave off inflammable gases, so also this wood, when heated, gives off inflammable gases; and when we say, in ordinary language, that a piece of wood is burning with flame, our language is not strictly correct; we should rather say that the heated wood gives off gases, and that those gases burn with flame,—and they burn with flame you perceive on the surface of the wood where they are discharged into the air, much in the same manner that the gas of the coal heated in the tobacco pipe burnt at

the other end of the pipe where it was discharged into the air. Now you will observe that where the piece of wood is subjected to heat, and more particularly where it is subjected to the hot flame of the burning gases surrounding it, it becomes blackened, or charred, or converted into charcoal. And the point of interest in connection with this charring process is that it does not take place where the wood itself, or the partly burnt wood, comes into contact freely with the air; but that it takes place where the wood is separated from the air by these burning gases. Where the wood is subjected to the heat of the burning gases, or to heat of any kind, and is kept out of contact with the air by the burning gases, or by some other means, there it becomes charred or converted into charcoal. But where the gases are burnt out, the charred residue, now left in contact with the air, quickly disappears, leaving only the white ash of which we spoke a moment ago. The same principle is made use of in the production of charcoal for manufacturing purposes. When manufacturers want to produce charcoal, they resort to one or other of two principal methods. One of these methods is to heat the wood to redness in an iron box or oven, entirely excluded from the air, with the exception of a pipe allowing the gases to escape; and after these gases have been driven off through the pipe, nothing is found left in the iron box or oven but a quantity of charcoal. Another way of making charcoal consists in piling the wood up into a large heap, and setting fire to it. By this means the outside wood, in contact with the air, gets burnt away to a greater or less extent; but the inside wood, being simply heated by the burning which is taking place upon the outside of the heap, does not get burnt away, but gives off its gases which burn on the outside; and what is left in the inside is this substance—charcoal, produced by the action of heat upon wood out of the access of air. Now if you examine a piece of charcoal obtained in one or other of these ways, and compare it with the wood out of which it was produced, you will observe that in the conversion of a particular piece of wood into a corresponding piece of charcoal, there has been an appreciable shrinking or loss of bulk; so that the resulting charcoal is considerably less in size than the original wood. It is also very much less in weight than the original wood; or, in the course of the process of its manufacture, there has been a certain shrinking in bulk, and a very much greater diminution of weight. But you will observe that the resulting charcoal presents exactly the form of the original piece of wood; so that you can recognise in it the stem and

branches and knots of the wood, the bark, and the pith, and even the longitudinal fibres and concentric laminæ of which the wood was constituted. From the circumstance, then, of charcoal, having these characters, being produced from wood by the driving away of certain of its component parts, so as to leave the charcoal behind, we come to the conclusion that wood is a substance partly composed of charcoal; or in other words, that charcoal is one of the constituents of wood.

But the charcoal obtained from wood is not itself a pure substance; it is contaminated, for instance, with the ashes of the wood; and, accordingly, when we burn the charcoal away these ashes are left as a white residue. In its pure state the black combustible matter of the charcoal is known by the name of "carbon," and we say accordingly that charcoal is an impure form of carbon. Now this substance, "carbon," in its pure state, is what chemists call a "simple substance," that is to say, a substance which they have not yet succeeded in breaking up, or resolving into two or more different kinds of substance. Wood, on the contrary, is a compound substance; and, when subjected to the action of heat, breaks up into charcoal, which remains behind, and certain gaseous products which are driven off. We take away something from the wood which is not wood, and thereby leave charcoal. But with regard to this substance—charcoal, or rather with regard to carbon in its pure state,—we cannot take anything away from it but carbon, and we cannot alter it in any way by the taking away of something from it, so as to leave anything but carbon. It is a substance which we may alter by adding something else to it—by combining something else with it—but which we cannot alter by taking anything else away from it. Therefore, in practical effect, if not in actual fact, carbon is a simple substance. It is a substance which has not yet been decomposed, and is not, so far as our present knowledge goes, decomposable into two or more different kinds of substance.

Now charcoal is not only a constituent of wood, but also of hay and corn, and indeed of vegetable produce generally. [A bundle of hay and a glass jar of corn were exhibited on the platform.] You know that hay has the property of undergoing by itself, under certain conditions, a process of heating, which sometimes results in its actually taking fire; and on cutting into a haystack, it is not an uncommon occurrence to find the interior portion of the stack completely charred by the heating which has taken place. Much in the same manner, then, that wood charcoal is produced by the heating of wood in heaps, pur-

posely set fire to—so is hay charcoal produced by the spontaneous heating of hay in haystacks; access of air to the interior being, in both cases, more or less completely prevented. And in the same way, if we take wheat grain and expose it to the action of heat, out of access of air, we get the grains completely charred or converted into charcoal. Here we have some wheat charcoal, presenting the form of the original grains of wheat—just as wood charcoal and hay charcoal present the forms of the original wood and hay respectively.

But it is important, in reference to the rest of the story I have to tell you this evening, that we should know, not only that vegetable produce—wood, and hay and corn—contain charcoal, but that we should be able also to form some notion of the amount of charcoal or carbon which they contain.

Now it is found that pure dry woody matter contains very nearly half its weight of carbon. It contains in reality 45 parts in 100, or, as we say, 45 per cent. If it contained 50 parts in 100, that would be exactly half its weight; but it does not contain quite this, but only 45 instead of 50 parts in 100. Now, if we pass from the consideration of pure woody matter to the consideration of other forms of vegetable produce, such for instance, as starch, of which here is a specimen, we find that starch contains exactly the same proportion of charcoal as woody matter; and that sugar, of which here is a specimen, contains very nearly the same proportion. Only a few lectures back, Professor Roscoe showed you that when sugar was acted upon by a certain chemical agent, it underwent a great swelling up, and became changed into a black spongy mass of charcoal, one of the constituent parts of the original sugar. And the proportion of charcoal, I repeat, in starch and sugar, is the same or very nearly the same as the proportion in pure woody matter. But we are acquainted with other vegetable substances which contain a much larger proportion of charcoal; such substances, for instance, as rosin and turpentine, and the oils expressed from seeds and fruits, as linseed oil, cabbage seed oil, and olive oil, &c. All these substances contain a much larger proportion of carbon than is contained in wood; and when they are set on fire, the smoke or soot they evolve in burning is some evidence to you of the large proportion of carbon which they originally contained. Now, just as certain vegetable products contain more carbon than wood, so there are other products which contain less; and among these I may refer to the different acids, or sour substances, which are found more particularly in the juices of

unripe fruit. There, for example, is a fine specimen of tartaric acid—an acid which exists in the juice of the grape, and is produced on a large scale, in wine-growing countries, in the process of converting the juice of the grape into wine. In the same way we meet with citric acid in the juice of lemons, and other vegetable acids in other vegetable juices. Now all these vegetable acids contain a smaller proportion of carbon than is contained in wood. But having regard to the fact that the great mass of vegetable produce is composed of woody matter, or of substances such as starch and sugar, having substantially the same composition as wood; and having regard further to the circumstance that, of other vegetable products, some of them contain a larger and some of them a smaller proportion of carbon than is contained in wood, it results that the amount of carbon contained in woody matter may be taken as a fair representative of the amount of carbon contained in vegetable produce generally, viewed as a whole. We may say, then, that the dry organic substance of a growing plant contains on an average about 45 parts in 100, or rather less than half of its weight of charcoal.

Now it is found that on an acre of meadow land, or arable land, or wood land, there are produced in the course of a single season several thousand pounds weight of vegetable produce, containing not unfrequently as much as two thousand pounds weight of charcoal; while the charcoal of an average crop may be taken at over 1,600 pounds, or nearly three-quarters of a ton per acre. In illustration of the large quantities of vegetable matter, and of its constituent carbon, produced annually on an acre of land, let me call your attention to the table before you, which shows the numbers deduced by Messrs. Lawes & Gilbert, from their many determinations of the quantities and compositions of actual crops of wheat, barley, and oats, as representing the average weights of produce obtained under the ordinary system of rotation of crops and moderately good farming.

	Wheat.	*Barley.*	*Oats.*	
Gross produce	4,800	4,580	4,172	Pounds per acre.
Dry organic matter	3,869	3,714	3,328	
Carbon	1,734	1,663	1,495	

From results obtained then, on Mr. Lawes' experimental farm at Rothamstead—a farm conducted for the purpose of knowledge and not for the purpose of profit—Mr. Lawes and Dr. Gilbert have arrived at the conclusion that, taking one year with another, the average weight of wheat, including grain and straw, produced

from an acre of land in a single season, amounts to 4,800 pounds. But the gross produce, as it is removed from the land, still contains, although seemingly dry, a considerable proportion of water; and if from the weight of gross produce there be deducted the weight of water which it contains, and if from the resulting weight of perfectly dry substance there be further deducted the weight of mineral matter or ash which it yields when burnt, there will be left 3,869 pounds as the weight of dry organic matter, and 1,734 pounds as weight of carbon contained in this organic matter. Similarly with regard to barley, the average weight of dry organic matter is 3,714 pounds per acre, including 1,663 pounds of carbon; while with regard to oats, the average weight of dry organic matter is 3,328 pounds per acre, including 1,495 of carbon. From results of this kind then, obtained in the cultivation of ordinary crops grown in a single season, you may form some notion of the large amounts of charcoal or carbon accumulated somehow in vegetable produce. And when we pass to the consideration of vegetation, not as we see it here, but as it manifests itself in the luxurious growth of tropical climates, the amounts of produce, and consequently of carbon contained in the produce, become yet more astounding. The celebrated naturalist and traveller, Humboldt, among his experiences in South America, records the existence there of forests so huge and so thick that monkeys might run on the tops of the trees for a hundred miles in a straight line, without a single break. And the millions of tons of dry wood, capable of being furnished by these forests, are composed, we know, to the extent of nearly half their weight, of charcoal! You perceive, then, that the growing plant, whether large or small, tree of the forest or grass of the field, may be regarded by us simply as a contrivance for producing carbon.

Reverting once more to the case of crops that are grown in a single season, it is evident that we remove from the land at the end of the season, several thousand pounds weight of vegetable produce which did not exist in the form of vegetable produce a few short months previously. Nevertheless the actual substance, or weight of matter, constituting this produce must have existed before the growth of the crop, although in a very different form. The several thousand pounds weight of wheat and barley and oats, grown on an acre of land in a single season, were not produced out of nothing; but were produced out of many thousand pounds weight of something pre-existing at the beginning of the season in the form of certain very different kinds of matter, out of which this matter of wheat and barley and oats was somehow constituted.

In the same manner, when, in course of time, the acorn grows into a tall oak tree, the several tons of matter, which go to compose the woody tissue of the full-grown oak, were not produced out of nothing, but out of many tons of matter which existed, though in a different form, before the acorn was even planted; and which have been accumulated, and transformed into woody matter, by the plant or tree, during the period of its many years growth. For the matter or substance of which the grown oak is finally composed, was not furnished by the acorn, but was furnished to the acorn, or young plant springing from the acorn, by external and very different forms of pre-existing matter. The problem then which I wish to put to you is this—what is the external matter or substance out of which the matter of wheat and barley and oats and hay and wood is ultimately produced? And more particularly, what is the sufficiently abundant substance containing carbon, out of which the carbon of all this vegetable produce is accumulated? for I need scarcely tell you that this carbon can only be got from some substance already containing carbon. Iron, you know, can only be produced from iron stone, or matter containing iron; copper can only be produced from copper ore, or matter containing copper; and in the same way, it is evident that the carbon of vegetable produce can only be obtained from matter containing carbon. What, then, is the primitive matter, containing carbon, out of which, in the course of the growth of the plant, this carbon of vegetable matter is ultimately produced?

It is well known that in forest lands, there exists a large amount of rich vegetable mould, the produce mainly of the decay of leaves; and this vegetable mould, which has received the name of *humus*, is found to be exceedingly rich in carbon. Further, richly carbonaceous vegetable matter of much the same kind is found in a sod of grass turf; and again matter of a not dissimilar kind is commonly added to arable land in the form of farmyard manure. Now, until about thirty years ago, the prevalent notion was that the carbon of vegetable produce was furnished to the plant by the carbonaceous matter of the soil called *humus*, or by matter of a similar nature. The vegetable matter of the growing plant was conceived to be formed out of pre-existing vegetable matter; and plants, like animals, were thus supposed to live upon food more or less resembling in composition the tissues or parts of the plants and animals respectively nourished. Now, notwithstanding the inadequacy of this notion, and notwithstanding its discordance with well-known facts, and with facts that had been for a long time well-known, it prevailed for very many years almost

without question. About thirty or more years ago, however, the consideration of eminent agricultural chemists both in England and in France was directed to this view of the subject, and very serious doubts of its truthfulness began to be entertained. But the notion was not ultimately exploded until the year 1840, by the celebrated German chemist, Liebig. Now I do not propose to take you over all the arguments which may be employed to show the inadequacy of this *humus* theory to account for the accumulation of carbon in plants; but I will direct your attention for a short time to some of the most prominent reasons only. First of all it is probable that in certain rich soils there does exist an amount of humus, or such like vegetable matter containing a quantity of carbon sufficient to furnish the crop grown upon the soil, with the carbon which it ultimately contains. But this vegetable humus is exceedingly insoluble in water; and Liebig made the curious calculation that if all the rain, that falls upon the land during the period of the growth of the crop, were to remain upon the land and to dissolve as much of this humus matter as it is capable of dissolving, so as to become thoroughly saturated with humus; and then, if all this water, so saturated with humus, instead of draining away, as we know that most of it does, and evaporating from the surface, as we know that much of it does,—it all of this so saturated water were absorbed into the tissues of the plants, nevertheless there could not be dissolved in this water, and so supplied to the plant, a sufficient quantity of humus to furnish the quantity of carbon ultimately found in the crop. This of course does not amount to a demonstration that the plant cannot get its carbon from the humus of the soil; it is only a demonstration that the plant cannot get its carbon from this humus by the only process of absorption of which we have any knowledge; and accordingly it comes to this, that if plants do acquire their carbon from humus, they must get it therefrom in a manner with which we are totally unacquainted. But another argument, and a much more striking one, has reference to the fact, that the carbon of the crop may be increased two-fold, and even three-fold, by adding to the soil matters which contain no carbon whatever. And this is very well shown in the table before you, which records some more of the results of Messrs. Lawes and Gilbert's work at Rothamstead. This table gives an account of experiments made on a tolerably large scale of experimental farming during the year 1868 and the 16 years preceding, in the case of wheat, making 17 years altogether; for 1868 and the 16 years preceding, in the case of barley; and for 1868 and the 12 years preceding, in the case of hay:—

Rothamstead Field Experiments, 1868.

Results in Pounds per Acre.

Gross Produce.

	Wheat. 17 *years.*	*Barley.* 17 *years.*	*Hay.* 13 *years.*
Unmanured	2,434	2,532	2,558
Mineral Salts	2,912	3,260	3,914
Do. + Ammonia	6,394	5,821	5,921
Farmyard Manure......	6,059	5,903	4,804

Dry Organic Matter.

Unmanured	1,963	2,054	1,995
Mineral Salts............	2,347	2,645	3,053
Do. + Ammonia	5,149	4,720	4,618
Farmyard Manure......	4,883	4,788	——

Carbon.

Unmanured	880	920	902
Mineral Salts............	1,052	1,185	1,380
Do. + Ammonia	2,308	2,115	2,088
Farmyard Manure......	2,183	2,341	——

For the purpose of these experiments, considerable strips of land have been treated every year, each strip in exactly the same way, for 17 years continuously, up to and including the year 1868; and indeed the experiments have been similarly carried on, and with similar results, up to the present year, 1871; and are likely to be similarly carried, on with similar results, for a good many years yet to come. And I would call your attention simply, as time is getting on so rapidly, to the case of wheat. You will then be able to make out for yourselves what were the results of the similar experiments made with the crops of barley and hay. Messrs. Lawes and Gilbert have found that, taking the average of these 17 years, the gross amount of produce removed from an acre of continuously unmanured land, in the case of wheat, was 2,434 lbs., and that when from this gross produce they subtracted the amounts of water it contained, and of ash which it yielded, there remained 1,963 pounds of dry organic matter; and when they came to analyse these 1,963 pounds of dry organic matter, they found them to contain 880 pounds of carbon. And this, mind, is the average produce of 17 years continuous growth of wheat, on land to which nothing whatever was added. Now to a similar strip of land Messrs. Lawes & Gilbert added every year a certain quantity of mineral matter, correspond-

ing to the ashes yielded by each successive crop removed; and on the strip so treated, the amount of gross produce was found to be increased from 2,434 pounds to 2,912 pounds, the amount of dry organic matter to be increased from 1,963 pounds to 2,347 pounds; and the amount of carbon to be increased from 880 pounds to 1,052 pounds. Now to another slip of land they added year by year exactly the same quantity of mineral matter, and in addition, a considerable quantity of ammonia salts,—the ammonia salts and mineral matter being alike absolutely free from carbonaceous organic matter. And in the case of this strip, they found that the amount of gross produce was increased to the surprising extent of 6,394 pounds, while the amount of dry organic matter was increased to 5,149 pounds, and the amount of carbon to 2,308 pounds. These results, you will observe, are fully as high—in most cases indeed somewhat higher—than are results obtained on a fourth strip of land, supplied year by year with an abundance of farm-yard manure, containing not only the mineral matter and ammonia added to the third strip, but rich also, as you know, in carbonaceous organic matter. It is inconceivable then that the plant should acquire its carbon from these organic matters of the soil, seeing that the amount of carbon in the crop may be increased twofold and in some cases nearly threefold, by adding to the soil substances such as mineral salts and ammonia which are entirely free from organic matter.

And this table further illustrates another point. We have admitted that the amount of humus or carbonaceous vegetable matter existing in the soil, might in some cases be sufficient to furnish the organic matter and the carbon for a single year's crop; but you observe that these 880 lbs. represent the average amount of carbon which has been produced for 17 years, and up to the present time, 21 years in succession; and which now seems to undergo from year to year no appreciable decrease. So that, although it is conceivable that the amount of humus in the soil might furnish the amount of carbon contained in a single crop, it is quite inconceivable that the original humus in the soil could furnish the carbon contained in a succession of crops for 17 years consecutively, and for the several years beyond that to which the experiment has now been carried, and for the indefinite number of years to which it will continue to be carried.

A still more cogent argument against this notion of the origin of the carbon of vegetation directly from organic matter in the soil, is afforded by the fact, established both by experiments specially made, and by the observation of nature, that plants and crops

have been, and in many cases habitually are, grown upon soils which are either absolutely free, or which are practically, and to all intents and purposes, free from organic vegetable matter. Very many such experiments have been made by the French chemist, Boussingault, who has grown plants from seeds in artificially prepared soils, which had been subjected to a red heat, and from which the whole of the organic carbonaceous vegetable matter had been so removed and burned away; and yet the plants have not only grown in these soils, but have thriven and arrived at maturity. It is found, moreover, that many plants flourish best, in a state of nature, upon soils which, if not like the experimental soils of Boussingault, absolutely free from organic matter, are yet to all intents and purposes free. Thus, according to Darwin, rich harvests of maize are yielded in the interior of Chili and Peru by soils consisting of the merest quicksand, never enriched by manure. According to Colonel Campbell, the soil of the cinnamon gardens at Colombo, and where else the tree is cultivated, is pure quartz sand, as white as snow. Dr. Schleiden, again, observes that the oil palms of the western coast of Africa are grown in moist sea-sand; and that from the year 1821 to the year 1830, there were exported, as produce of these palm-trees, into England alone, 107,118,000 lbs. of palm oil, containing 76 million lbs. or 32 thousand tons of carbon; these thousands of tons of carbon being furnished by trees grown in a soil that was practically free from organic or carbonaceous matter of any kind whatever.

The only further argument with which I will trouble you is based on the observation that when plants are grown upon soils actually containing organic vegetable matter, so far from this vegetable matter in the soil being used up or decreased by any feeding of plants upon it, it is very much increased; so that the more vegetation we get from the surface the more humus we get accumulated in the soil; and we say, therefore, that so far from humus being the cause of vegetation, vegetation, on the contrary, is the cause of humus—the humus being produced chiefly by the decay of matter formed by vegetation.

I think, then, I have now brought before you, not all the arguments which might be adduced, but a sufficient number of them to satisfy you that the quantities of carbon accumulated in the crop or tree are not derived from carbonaceous matter existing in the soil; and seeing, in this way, that the solid substance of the earth does not suffice to furnish the carbon required, our attention is next directed to the liquid water which falls upon the earth,

as a possible source of all this carbon. Now water—pure water, that is to say—is a substance which itself contains no carbon, and therefore cannot furnish any carbon to the plant. But certain natural waters are found to contain carbon in small quantity. For instance, the drainage water of peat bogs, and land-drainage water in general, contains a certain amount of carbonaceous organic matter derived from the land; but we have already seen that the land does not contain enough of this organic matter to furnish the carbon of vegetation directly, and cannot therefore furnish it indirectly through the intervention of water, taking up organic matter from the land.

But we find that rain water does contain carbon derived from another source. The rain, in falling through the air, acquires different impurities or additions from the air; and more especially it takes up a certain carbonaceous constituent of the air, on which I shall have to dwell more particularly in a minute or two's time. And I am not merely speaking of the rain which has fallen in great cities like this, and has so become contaminated with the carbonaceous soot and smoke of imperfectly burnt coal; but I am speaking of rain wherever it falls, whether on land or ocean, in town or country, at the end of a period of drought when the air is foul, as at the end of a period of wet, when it has been washed clean by continuous showers. Pure water I have said, is quite free from carbon in any form whatever. But all water that has been left in contact with the air, and especially water that has been condensed from and fallen through the air, contains, in small proportion, a particular definite compound of carbon, namely, carbonic acid, very different indeed in its nature from the indefinite compounds of carbon we have hitherto spoken of under the name of humus and vegetable organic matter.

In this way our attention is necessarily directed to the air as a possible source of all the millions of tons of carbon that are accumulated in forest trees and annual crops, growing on extensive areas of land. And although at first sight it must strike us all as being improbable—scarcely, we should think, possible—that any such quantity of solid carbon could be got from the fresh, transparent, intangible, fleeting air, yet, when we consider that upon setting fire to a heap of wood, or of the charcoal produced from wood, and letting it go on burning, it is mainly resolved into matters which are dispersed into the air, and are themselves aerial, we begin to perceive that the improbability is not in reality so great as at first it appears. When we burn, however large a quantity of wood, or of the charcoal produced

from wood, there is nothing, you know, left behind but an insignificant quantity of ashes; there is no solid body formed; there is no liquid body formed; there is nothing but an aerial body formed, which is discharged into the air. Now this aerial body used actually to be called air—fixed air, to distinguish it from ordinary atmospheric air — but is now-a-days called carbonic acid gas. This carbonic acid gas is possessed of many very curious properties, and is more especially characterised by two properties, to which I am desirous of calling your attention. The first of these is the property which it has of extinguishing the flame of any burning body. On introducing a lighted gas jet into this bottle of carbonic acid gas, the flame, you observe, is at once extinguished. [An experiment illustrated this fact.] Another property of carbonic acid gas is the property it has of combining with lime, to produce carbonate of lime, or chalk. Now lime is a substance which dissolves in water to form a clear transparent liquid; but chalk is a substance that will not dissolve in water. You may observe, when you go to the sea-side, that the sea-salt remains dissolved in the water, while the sea-sand remains undissolved upon the shore. Now lime, like salt, dissolves in water, though, indeed, to a much less extent than salt, to furnish a perfectly bright solution known as lime-water. Chalk, on the other hand, like sand, is a substance which does not dissolve in water, but remains simply mixed up with it for a time, in the form of a white milky opaque liquid. The property, then, which carbonic acid has of combining with lime to produce chalk, is manifested to you in this way—that upon adding our clear lime water to the carbonic acid in the bottle, carbonate of lime or chalk is formed, and this chalk, not being soluble in water, is deposited so as to form the milky liquid which you see we have now produced. [Experiment made.] This other bottle also contains carbonic acid, but mixed with a considerable excess of air; so that in this case, there is not a sufficient amount of carbonic acid present to cause the extinction of flame. When I put in the gas-flame you see that it continues burning. But that the bottle really does contain some carbonic acid, I can show you by adding in this case also our lime water; and now, on shaking up the bottle, the lime water is at once rendered milky. You see in this way, we have two tests for carbonic acid. When the carbonic acid exists in a large proportion, it has the property of rendering lime water milky and also of extinguishing the flame; but when the proportion of carbonic acid is not sufficient to extinguish flame, we are able, nevertheless, to recognise its presence

by the property it has of converting our clear lime water into an opaque white mixture of chalk and water.

Now I told you a few moments ago that the aerial substance into which solid charcoal was converted, when it underwent the process of being burnt in air, was carbonic acid gas. And, accordingly, when I put some pieces of red hot charcoal into this upright glass tube, through which a gentle current of air is being blown, so as to keep the charcoal burning, and when I cause this same air, now charged with the aerial matter furnished by the burning charcoal, to bubble up through lime water, you perceive the lime water is quickly rendered milky, showing you the formation of carbonate of lime or chalk, a substance producible only from lime by the addition of carbonic acid to it. [Experiment made.]

I want next to call your attention for a moment to what takes place in the act of burning. Ordinary atmospheric air consists substantially of two distinct kinds of air or gas—one is called nitrogen and the other oxygen. Now when our charcoal or carbon burns in the open air, or in the tube through which we are blowing a current of air, that carbon enters into combination with the oxygen of the air, and forms a compound of oxygen and carbon, which is, indeed, sometimes called oxide of carbon, but more commonly, as I have said, carbonic acid. If, instead of burning our carbon in the air, which contains only one-fifth of its bulk of oxygen, we burn it in pure oxygen, it burns with greatly increased brilliancy, but furnishes exactly the same product, namely, carbonic acid. Here we have the chalk, which we produced a moment ago, by taking lime water and adding to it the carbonic acid we made by combining our carbon or charcoal with the oxygen of the air; and here we have some charcoal that is already ignited; and on passing the pure oxygen gas over it, you observe the very greatly increased brilliancy with which, under these circumstances, it burns. We next cause the air which is left by this burning of the charcoal in oxygen, to bubble up through lime water; and the abundant presence in it of oxide of carbon, or carbonic acid gas, is at once manifested to you by the immediate deposition of carbonate of lime or chalk. [Experiment made.] I venture to impress upon your attention the fact that carbonic acid gas is a compound of the solid substance carbon with the aerial or gaseous substance oxygen; and that when carbon or charcoal burns in ordinary air, it unites with the oxygen of the air to form the aerial substance, carbonic acid gas, which is discharged into the air.

Now, if we reflect for a minute or two, we shall see that inasmuch as wood and charcoal, and I may add coal (although we are not talking about coal on the present occasion), when they are burned, produce the aerial substance, oxide of carbon, or carbonic acid; and inasmuch as they discharge this carbonic acid into the air; it is a matter of necessity that the air itself should contain some carbon in this particular form. And not only is it a matter of necessity that it must contain, but it is also a matter of easy experimental demonstration that it actually does contain this aerial compound of carbon, namely, carbonic acid. One rough way of establishing the fact is this:—If we take some clear, transparent, colourless lime water, and pour it into a dish, and expose it to the air for several hours, the top layer of the lime water in contact with the air, gradually becomes converted into an opaque white scum of chalk; and chalk, we know, is producible only from lime, by the acquisition of carbonic acid, which can in this case have been acquired from no other source than from the air with which the surface of lime water was in contact. That the air, then, must contain some carbonic acid is a matter of argument; and that it does contain some is a matter of experimental fact.

But although the air does, beyond question, contain carbon in the form of carbonic acid, the proportion that it contains is exceedingly small; as you may infer from the length of time we require to keep lime water exposed to the air, in order for it to acquire a thick scum; and from the circumstances that we may even blow a current of air through lime water for a considerable time, without producing any sensible effect. [Further experiments.] We are now blowing ordinary air through this lime water; and I might go on blowing for a great length of time, before I should get any appreciable turbidity. This shows you that although the air does contain carbonic acid, it must contain it in an exceedingly small proportion. We require, then, to know what this proportion is. Now it is found that the amount of carbonic acid gas in the open air varies within a certain range, but that it amounts on the average to somewhat less than one-half part in a thousand parts by volume: or we may say more accurately that it constitutes four parts in ten thousand. Here the composition of the air is written up:—

COMPOSITION OF AIR.

$\frac{1}{5}$ Oxygen	210		Parts per 1000.
$\frac{4}{5}$ Nitrogen	790		
Carbonic acid .... nearly	$\frac{1}{2}$		

Nitrogen gas 790 parts, or about four-fifths; oxygen 210 parts, or about one-fifth; and carbonic acid gas not quite one half part. If it contained exactly one-half part, that would of course be five parts, instead of only four parts, in 10,000. Now the expression of four parts in 10,000 does not convey a very definite idea to the mind, but I may perhaps render it more definite to you in this way. Imagine four farthings among ten thousand farthings, or, what comes to the same thing, imagine one penny piece among two thousand five hundred penny pieces. If you were to take 2,500 penny pieces and pile them on the top of each other you would produce a column of pence some 15 or 16 feet high—about as high as this rod, and considerably more than twice the height of the tallest man in the room—and if from such a pile of 2,500 pence you were to remove one penny, that would represent to you the bulk of carbonic acid gas contained in a similar column of air: that is, the one part of carbonic acid in 2,500 parts of air, or, of course, four parts of carbonic acid in 10,000 of air. But although the proportion is exceedingly small, a little consideration will suffice to show us that the absolute quantity is exceedingly great. I have said that the proportion is four parts of carbonic acid in 10,000. Now, consider for a moment what is the quantity existing in the air of a moderately sized room. A room 25 feet long, 25 feet broad, and 16 feet high, would hold 10,000 cubic feet of air, containing, of course, four cubic feet of carbonic acid gas. And these four cubic feet of carbonic acid gas would weigh 2,465 grains, and contain 607 grains of charcoal—that is to say, the quantity of charcoal I now hold in my hand (about the size of an egg). This Town Hall holds, in round numbers, about 150,000 cubic feet of air, and, consequently, the amount of carbonic acid contained in it will be fifteen times four, or 60 cubic feet; and the amount of charcoal contained in this carbonic acid will be fifteen times 607 grains, or the weight of the bundle of charcoal, considerably more than a pound and a quarter, I now hold in my hand. And when we pass from the consideration of the air in rooms, small or large, to the consideration of the air pressing everywhere upon the surface of the earth, we shall get to results great almost beyond conception. You know that the weight of air overlying every square inch of the earth's surface is 15 lbs., and that this is what we mean by saying, as we commonly do, that the atmospheric pressure is 15 lbs. on the square inch. Now, 15 lbs. on the square inch is 2,160 lbs. on the square foot; so that every square foot of the earth's surface has overlying it 2,160 lbs. of air, and these

2 160 lbs. of air contain about $1\frac{1}{2}$ lbs. of carbonic acid gas, equivalent to very nearly half a pound of carbon. I showed you a few minutes ago that there are produced, in many cases, from an acre of land, some 2,000 lbs. of carbon in a single season. Now, reckoning from feet to acres, we find that not merely at the first instant of the growth of the crop, but that during every instant of the period of its growth—at the end no less than at the beginning—there is overlying the acre of land furnishing those 2,000 lbs. of carbon some 20,000 lbs. of carbon in the form of carbonic acid, existing, though in such small proportion, in the air. Calculating in this way, we find that the amount of carbon existing in the atmosphere, in the form of carbonic acid gas, is not only enormous in its absolute quantity, but that it is far in excess of the wants of vegetation, and far in excess, moreover, of the quantities of carbon contained in all living beings, both plants and animals, existing on the surface of the earth, and in inflammable carbonaceous minerals, such as coal, which exist buried beneath the surface.

In this way, then, we come to the conclusion that by their contact with the air, plants are at any rate afforded the opportunity of getting that carbon, which constitutes so large a proportion of their structure. The question now is, do they avail themselves of the opportunity afforded them—do they actually absorb carbonic acid gas from the atmosphere, and extract the carbon of the gas which they absorb. Now, the evidence on this point dates from the latter end of the last century; when it was ascertained by the older chemical philosophers, and more particularly Dr. Priestley, and by Saussure and Sennebier, that when growing plants are exposed, under the influence of sunlight, to air containing carbonic acid, they do as a matter of fact absorb some of this carbonic acid; and, that having absorbed it, they do not discharge it again into the air, but instead discharge only its one constituent oxygen; the necessary inference being that its other constituent, carbon, is retained in their tissues. Here you have an imitation of one of these early experiments, showing the removal of carbonic acid from, and the restoration of oxygen to, a confined amount of air, by means of a fresh sprig of mint or parsley. [Experiment.] Of late years, the subject has been investigated with great care and elaboration by the French chemist Boussingault, who has shown not merely that plants have this property of absorbing carbonic acid from the air, and of discharging the constituent oxygen of the gas into the air and retaining the constituent carbon of the gas in

their tissues, but that they do this with extreme rapidity. The mode of experimenting which he adopted is illustrated to you here. Taking a growing plant, such as this, he enclosed one or more branches of the plant in a glass vessel, and through that glass vessel passed a current of air, which was subjected to analysis both before and after its passage through the vessel. [Experiment to show the process of sucking air through a globe holding the branch of a growing plant.]

I cannot trouble you at this late hour with the details of his experiments, but will call your attention only to one or two of the results. In the case of some oleander leaves, enclosed in a glass globe of this kind, he found, by measuring the leaves and analyzing the air passing over them, that under exposure to sunlight, there was an absorption of carbonic acid from the air at the rate of 56½ cubic inches. or a fixation of carbon at the rate of 11½ grains per hour, per square yard of leaf surface exposed, showing the extreme rapidity with which the absorption of carbonic acid from the air and the retention of its carbon actually took place. Moreover, he made a great number of other experiments, that I cannot refer to in detail, which established not merely the general fact that plants can absorb carbonic acid gas from the air, and can discharge the oxygen and retain the carbon of the gas so absorbed; but, operating with seeds, and more particularly with peas and vetches, and growing them in artificial soils quite free from carbon, he found that the entire weight of the carbon ultimately accumulated in the grown plant was identical with the weight of carbon contained in the carbonic acid gas which the growing plant had absorbed from, and the oxygen of which alone it had discharged back into the atmosphere. In this way, then, Boussingault established the important fact that plants acquire their carbon from the carbonic acid of the abundant ever-changing air, in which they are grown.

We have thus considered the source from which the carbon of vegetation is obtained. But we have yet another point to consider, and that is—what becomes of it? Now, a little consideration, I think, will show you, that just as the carbon of vegetation is produced from the aerial substance, carbonic acid gas, so the destiny, if I may so say, of the carbon of vegetation is to be reconverted into this same aerial substance. First of all, let us see what becomes of the most abundant of vegetable products, namely, wood. You know that a great deal of fresh wood is put to no intermediate use, but is at once chopped up for the fire; and when this wood is burned, its carbon combines with the oxygen of the

air, and is so re-converted into carbonic acid. Again, a considerable quantity of wood is manufactured into charcoal, and this charcoal is then burned and so converted into carbonic acid. And with regard to the diverse applications of wood, we know that much of it is made into furniture, and that this furniture does not last for ever, but finds its way from the best rooms to the attics, and at last to the fireplace. Wood is also used for the building of ships, and in the construction of houses; but in course of time, the ships get broken up, and the houses get pulled down, and the wood of both ships and houses becomes ultimately sold for firewood, and then the carbon of this wood gets burnt into the very carbonic acid from which it was long years before produced. In other cases, the wood or woody matter, although it never undergoes a process of actual burning, nevertheless undergoes an equivalent process of oxidation. At the present season, or but very recently, we had large falls of autumn leaves, and those leaves are still accumulated in many places, and undergoing not burning but decay. Now the process of decay consists really in a slow combination of the carbon of the leaves with the oxygen of the air, whereby carbonic acid is produced. Here we have some fallen leaves in a flask; the air of which you will find is now sufficiently charged with carbonic acid gas, produced by the union of the carbon of the decaying leaves with the oxygen of the original air, as to be no longer capable of maintaining the flame of a taper or gas jet. [Experiment.] The moment I introduce the taper you see that its flame is at once extinguished. Here again we have some sawdust which is undergoing the same process. The moist sawdust gradually undergoes decay; whereby the oxygen of the air is gradually absorbed and the carbon of the sawdust gradually converted into carbonic acid, so that the flame of the taper is in this case also at once extinguished. [Experiment.] And, indeed, woody matter of all kinds exposed to the weather, to the action, that is, of air and water, gradually undergoes decay or oxidation, and, if left to itself, crumbles away, and in course of time, disappears altogether, being converted into the invisible aerial matter carbonic acid.

When we pass from the consideration of wood to that of the hay and grain eaten by different classes of animals, and mark what becomes of all this food, we shall find that so much of it as is both eaten and made part of the blood and substance of the vegetable feeding animal, undergoes one or other of two principal changes. A large portion of it gets oxidised in the body of the vegetable-feeder, with production of carbonic acid, discharged principally

from the lungs in the act of respiration. Another portion gets accumulated in his body, whereby it is fattened and rendered fit to become the food of the flesh feeder. And when the flesh-feeding animal eats up the bodies of the vegetable feeders, their vegetable-derived fat and lean that becomes assimilated in his body is found to suffer there a speedy oxidation. Store animals, intended for food, increase gradually in weight; but hard-working animals, whether vegetable feeders like the horse, or mixed feeders like ourselves, or animal feeders like the hound, go on eating day after day, year after year, without any sensible increase of bodily weight—the carbonaceous matter of the food continually eaten, sufficing only to replace that continually destroyed in the process of gradual oxidation or burning away to which the substance of our blood and tissues is ever subjected, in order that the temperature and activity of our bodies may be maintained. Accordingly, we find the air expired from the lungs of both vegetable and animal feeders, to be charged with carbonic acid, produced by the oxidation of carbonaceous organic matter—furnished directly or indirectly by the vegetable kingdom, out of aerial carbonic acid, and restored by the animal back into the same carbonic acid. On breathing into this lime water for a little time [Experiment made] we have shortly a dense milky deposit of carbonate of lime, or chalk, produced—the carbonic acid, thus serving to convert the lime into chalk, being supplied by the oxidation within our bodies of carbonaceous organic matter, accumulated in the first instance by the growing vegetable. So that in the case of food consumed in our bodies, as in the case of wood consumed on our fires, the carbon of vegetable produce is directly or indirectly converted back into the aerial carbonic acid from which it was originally formed. I need only detain you a few minutes longer. When we burn charcoal in the fire, it evolves in the act of burning a considerable amount of heat. The temperature produced in this way varies considerably, accordingly to circumstances. We may have a fire in which the charcoal is just glowing, and the temperature comparatively low—hardly sufficient to raise a piece of metal to a visible red heat; and with another quantity of charcoal on the fire, urged by the blast of powerful bellows, we may obtain an intense degree of temperature, capable of melting that most difficultly fusible metal—wrought iron. Now, whether we obtain a high or a low degree of temperature depends mainly upon the rapidity with which we burn the charcoal. If we take a quantity of charcoal and burn it away slowly, it gives out its

heat over a length of time, and at no one instant is there a very high degree of temperature; but if we take that same quantity of charcoal and, setting it on fire, burn it rapidly away, we get a very high degree of temperature; so that the degree of temperature produced by the burning of charcoal depends upon the quantity of charcoal that is burned within a limited space and time. But if we take any quantity of charcoal, say an ounce, and burn it in one case very slowly, and in another case very quickly, and do this in a vessel surrounded on all sides by water, so that all the heat produced in the hour say, or, in the few minutes, shall be taken up and retained in the water, we shall find that the quantity of heat imparted to the water is exactly the same in both cases. So that whether we burn the charcoal quickly, so as to get a high temperature, or burn it slowly, so as to get a low temperature, the quantity of heat which that charcoal produces in burning, as measured by the quantity of water it is capable of heating through a given rise of temperature is exactly the same in both cases. And this is true, not only when we actually burn charcoal upon a fire, but in all cases of the conversion of carbon or charcoal into carbonic acid, by the act of oxidation. And indeed the temperature of our own bodies is maintained in a great measure by the slow oxidation, or quasi-combustion of carbonaceous matter going on within us. Whether, then, we burn our charcoal in an open fire rapidly, so as to produce a high temperature, or whether we burn it in our bodies slowly, so as to produce a low temperature, we find that for so much carbon converted into carbonic acid, there is exactly the same quantity of heat produced. For example—In burning one ounce of charcoal into about 3½ ounces of carbonic acid, a quantity of heat is evolved, sufficient to raise the temperature of 100 pounds, or 10 gallons of water ten degrees; and this, whether the act of burning takes place quickly or slowly, with production of a high or of a low degree of temperature. Now it is a well-established law in chemistry, established, I mean, by the careful examination of a great number of instances, that whenever heat is given out by the act of combination, as of charcoal and oxygen to produce carbonic acid, exactly the same quantity of heat is absorbed in the corresponding act of separation, as of charcoal and oxygen, out of carbonic acid. The conversion of carbon into carbonic acid, on the fire, is a burning process, attended with the evolution of heat. The conversion of carbonic acid into carbon and oxygen, in the tissues of a growing plant under the influence of the sun's rays, is an unburning process attended, not with an

evolution of heat, but with an absorption of heat from the solar rays: and it follows that there is just as much disappearance of solar heat in the production of the charcoal, as there is evolution of heat in the ultimate combustion of the charcoal produced. So that, you see, the quantity of heat which the charcoal eventually gives out in burning on the fire, is the exact equivalent of the quantity of solar heat which disappeared in the act of growth of the wood, from which the charcoal furnishing our fire was obtained.

THE UNCONSCIOUS ACTION OF THE BRAIN.

A LECTURE

BY DR. CARPENTER, F.R.S.;

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Delivered in the Hulme Town Hall, Manchester, December 1st, 1871.

MANY of you, I doubt not, will remember that I had the pleasure of addressing you in this hall some months ago, with reference to researches which I had a share in carrying on into the Depths of the Ocean; when I endeavoured to give you some insight into the conditions of the sea bottom as regards temperature, pressure, animal life, and the deposits now in process of formation upon it.

Now I am going this evening to carry you into quite a different field of inquiry, an inquiry which I venture to think I have had some share in myself promoting, into what goes on in the Depths of our own Minds. And I think I shall be able to show you that some practical results of great value in our own mental culture, as training and as discipline, may be deduced from this inquiry. I shall begin with an anecdote that was related to me after a lecture which I gave upon this subject about five years ago, at the Royal Institution, in London. As I was coming out from the lecture-room, a gentleman stopped me and said, "A circumstance occured recently in the North of England, which I think will interest you, from its affording an exact illustration of the doctrine which you have been setting forth to-night." The illustration was so apposite, and leads us so directly into the very heart of the inquiry, that I shall make it, as it were, the text for the commencement of this evening's lecture. The Manager of a bank in a certain large town in Yorkshire could not find a key which gave access to all the safes and desks in the bank. This key was a duplicate key, and ought to have been found in a place accessible

only to himself and to the assistant-manager. The assistant manager was absent on a holiday in Wales, and the manager's first impression was that the key had probably been taken away by his assistant in mistake. He wrote to him, and learned to his own great surprise and distress that he had not got the key, and knew nothing of it. Of course, the idea that the key, which gave access to every valuable in the bank, was in the hands of any wrong person, having been taken with a felonious intention, was to him most distressing. He made search everywhere, thought of every place in which the key might possibly be, and could not find it. The assistant-manager was recalled, both he and every person in the bank were questioned, but no one could give any idea of where the key could be. Of course, although no robbery had taken place up to this point, there was the apprehension that a robbery might be committed after the storm, so to speak, had blown over, when a better opportunity would be afforded by the absence of the same degree of watchfulness. A first-class detective was then brought down from London, and this man had every opportunity given him of making inquiries; every person in the bank was brought up before him; he applied all those means of investigation which a very able man of this class know how to employ; and at last he came to the manager and said, "I am perfectly satisfied that no one in the bank knows anything about this lost key. You may rest assured that you have put it somewhere yourself, and you have been worrying yourself so much about it that you have forgotten where you put it away. As long as you worry yourself in this manner, you will not remember it; but go to bed to-night with the assurance that it will be all right; get a good night's sleep; and in the morning I think it is very likely you will remember where you have put the key." This turned out exactly as it was predicted. The key was found the next morning in some extraordinarily secure place which the Manager had not previously thought of, but in which he then felt sure he must have put it himself.

Now, then, ladies and gentlemen, this you may say is merely a remarkable case of that which we all of us are continually experiencing; and so I say it is. Who is there among you who has not had occasion some time or other to try to recall something to his (or her) mind which he has not been able to bring to it? He has seen some one in the street, for instance, whose face he recognises and says, "I ought to know that person;" and thinks who it can be, going over (it may be) his whole list of friends

and acquaintances in his mind, without being able to recall who it is; and yet, some hours afterwards, or it may be the next day, it flashes into his mind who this unknown person is. Or you may want to remember some particular and recent event; or it may be, as I have heard classical scholars say, to recall the source of a classical quotation. They "cudgel their brains," to use a common expression, and are unsuccessful; they give their minds to something entirely different; and some hours afterwards, when their thoughts are far away from the subject on which they had been concentrating them with the idea of recovering this lost clue, the thing flashes into the mind. Now this is so common an occurrence, that we pass it by without taking particular note of it; and yet I believe that the inquiry into the real nature of this occurrence may lead us to understand something of the inner mechanism of our own minds which we shall find to be very useful to us.

There is another point, however, arising out of the story which I have just told you, upon which again I would fix your attention:—Why and how did the detective arrive at this assurance from the result of his inquiries? It was a matter of judgment based upon long practice and experience, which had given him that kind of insight into the characters, dispositions, and nature of the persons who were brought before him, which only those who have got that faculty as an original gift, or have acquired it by very long experience, can possess with anything like that degree of assurance which he was able to entertain. I believe that this particular power of the detective is, so to speak, an exaltation in a particular direction of what we call "common sense." We are continually bringing to the test of this common sense a great number of matters which we cannot decide by reason; a number of matters as to which, if we were to begin to argue, there may be so much to be said on both sides, that we may be unable to come to a conclusion. And yet, with regard to a great many of these subjects—some of which I shall have to discuss in my next lecture—we consider that common sense gives us a much better result than any elaborate discussion. Now I will give you an illustration of this which you will all readily comprehend. Why do we believe in an external world? Why do I believe that I have at present before me many hundreds of intelligent auditors, looking up and listening to every word that I say? Why do you believe that you are hearing me lecture? You will say at once that your common sense tells you. I see you; you see and hear me; and I know that I am addressing you. But i. once this

subject is logically discussed, if once we go into it on the basis of a pure reasoning process, it is found really impossible to construct such a proof as shall satisfy every logician. As far as my knowledge extends, every logician is able to pick a hole in every other logician's proof. Now here we have then a case obvious to you all, in which common sense decides for us without any doubt or hesitation at all. And I venture to use an expression upon this point which has been quoted with approval by one of the best logicians and metaphysicians of our time, Archbishop Manning; who cited the words that I have used, and entirely concurred in them, namely, that "in regard to the existence of the external world the common-sense decision of mankind is practically worth more than all the arguments of all the logicians who have discussed the basis of our belief in it." And so, again, with regard to another point which more nearly touches our subject to-night—the fact that we have a Will which dominates over our actions; that we are not merely the slaves of automatic impulse which some philosophers would make us—"the decision of mankind (as Archbishop Manning, applying my words, has most truly said) derived from consciousness of the existence of our living self or personality, whereby we think, will, or act, is practically worth more than all the arguments of all the logicians who have discussed the basis of our belief in it."

Now, then, my two points are these—What is the nature of this process which evolves, as it were, this result unconsciously to ourselves, when we have been either asleep, as in the case of the banker, or, as in the other familiar case I have cited, when we have been giving our minds to some other train of thought in the interval? What is it that brings up spontaneously to our consciousness a fact which we endeavoured to recall with all the force of our will, and yet could not succeed?

And then again:—What is the nature of this Common Sense, to which we defer so implicitly and immediately in all the ordinary judgments of our lives?

Now, in order that we may have a really scientific conception of the doctrine I would present to you, I must take you into an inquiry with regard to some of the simpler functions of our bodies, from which we shall rise to the simpler actions of our minds. You all know that the Brain, using the term in its general sense, is the organ of our Mind. That every one will admit. We shall not go into any of the disputed questions as to the relations of Mind and Matter; for the fact is that these are now coming to take quite a new aspect, from Physical philosophers dwelling so much

more upon Force than they do upon Matter, and on the relations of Mind and Force, which every one is coming to recognise. Thus when we speak of nerve-force and mind as having a most intimate relation, no one is found to dispute it; whereas when we talk about Brain and Mind having this intimate relation, and Mind being the function of the brain, there are a great many who will rise up against us and charge us with materialism, and atheism, and all the other deadly sins of that kind. I merely speak of the relation of the brain to the mind, as the instrument through which the mind operates and expresses itself. We all know that it is in virtue of the impressions carried to the brain through the nerves proceeding from the different sensory organs in various parts of the body, that we become conscious of what is taking place around us. And, again, that it is through the nerves proceeding from the brain that we are able to execute those movements which the Will prompts and dictates, or which arise from the play of the Emotions. But I have first to speak of a set of lower centres, those which the Will can to a certain extent control, but which are not in such immediate relation to it as is the brain. You all know that there passes down our backbone a cord which is commonly called the "Spinal Marrow." Now this spinal marrow gives off a pair of nerves at every division of the backbone; and these nerves are double in function—one set of fibres conveying impressions from the surface to the spinal cord, the other motor impulses from the spinal cord to the muscles. Now it used to be considered that this Spinal Cord (I use the term spinal cord, which is the same as spinal marrow, because it is just as intelligible and more correct) was a mere bundle of nerves proceeding from the brain; but we have long known that that is not the case, that the spinal cord is really a nervous centre in itself, and that if there were no brain at all the spinal cord would still do a great deal. For example, there have been infants born without a brain, yet these infants have breathed, have cried, have sucked, and this in virtue of the separate existence and the independent action of this spinal cord. Let us analyse one or two of these actions. We will take the act of Sucking as the best example, because experiments have been made upon young puppies, by taking out the brain, and then trying whether they would suck; and it was found that putting between the lips the finger moistened with milk or with sugar and water, produced a distinct act of suction, just as when an infant is nursed. Now how is this? It is what we call a "reflex action." I shall have a good deal to say of reflex action higher up in the nervous system, and therefore I must

explain precisely what we mean by that term. It is just this. There is a certain part of the spinal cord, at the top of the neck, which is what we call a ganglion, that is, a centre of nervous power: in fact the whole of the spinal cord is a series of such ganglia; but this ganglion at the top of the neck is the one which is the centre of the actions which are concerned in the act of sucking. Now this act of sucking is rather a complicated one, it involves the action of a great many muscles put into conjoint and harmonious contraction. We will say then that here is a nervous centre. [Dr. Carpenter made a sketch upon the black board.] These are nerves coming to it, branches from the lips; and these another set going to the muscles concerned in the movement of sucking from it. Thus, by the conveyance to the ganglionic centre of the impression made on the lips, a complicated action is excited, requiring the combination of a number of separate muscular movements. We will take another example—the act of Coughing. You feel a tickling in your throat, and you feel an impulse to cough which you cannot resist; and this may take place not only when you are awake and feel the impulse, but when you are asleep and do not feel it. You will often find persons coughing violently in sleep, without waking or showing any sign of consciousness. Here, again, the stimulus, as we call it, produced by some irritation in the throat, gives rise to a change in the nerves going towards the ganglionic centre, which produces the excitement of an action in that centre that issues the mandate, so to speak, through the motor nerves to the muscles concerned in coughing, which actions have to be united in a very remarkable manner, which I cannot stop to analyse; but the whole action of coughing has for its effect the driving out a violent blast of air, which tends to expel the offending substance. Thus when anything "goes the wrong way," as we term it,—a crumb of bread, or a drop of water finding its way into the windpipe, then this sudden and violent blast of air tends to expel it.

Now these are examples of what we call "reflex action"; and this is the character of most of the movements that are immediately concerned with the maintenance of the vital functions. I might analyse other cases. The act of breathing is a purely reflex action, and goes on when we are perfectly unconscious of exerting any effort, and when our attention is entirely given up to some act or thought; and even when asleep the act of breathing goes on with perfect regularity, and if it were to stop, of course the stoppage would have a fatal effect upon our lives. But most of these reflex actions are to a certain degree placed under the control of our Will. If it

were not for this controlling power of will, I could not be addressing you at this moment. I am able so to regulate my breath as to make it subservient to the act of speech; but that is the case only to a certain point. I could not go on through a long sentence without taking my breath. I am obliged to renew the breath frequently, in order to be able to sustain the circulation and other functions of life. But still I have that degree of control over the act of respiration, that I can regulate this drawing in and expulsion of the breath for the purposes of speech. This may give you an idea of the way in which Mental operations may be independent of the Will, and yet be under its direction. To this we shall presently come.

Now those reflex actions of the spinal cord, which are immediately and essentially necessary to the maintenance of our lives, take place from the commencement without any training, without any education; they are what we call "instinctive actions;" the tendency to them is part of our nature; it is born with us. But, on the other hand, there are a great many actions which we learn, to which we are trained in the process of bodily education, so to speak, and which, when we have learned them, come to be performed as frequently, regularly, methodically, and unconsciously as those of which I have spoken. This is the case particularly with the act of walking. You all know with how much difficulty a child is trained to that action. It has to be learned by a long and painful experience, for the child usually gets a good many tumbles in the course of that part of its education; but when once acquired it is as natural as the act of breathing, only it is more directly under the control of the will; yet so completely automatic does it become, that we frequently execute a long series of these movements without any consciousness whatever. You start in the morning, for instance, to go from your home to your place of employment; your mind is occupied by a train of thought, something has happened which has interested you, or you are walking with a friend and in earnest conversation with him; and your legs carry you on without any consciousness on your part that you are moving them. You stop at a certain point, the point at which you are accustomed to stop, and very often you will be surprised to find that you are there. While your mind has been intent upon something else, either the train of thought which you were following out in your own mind, or upon what your friend has been saying, your legs move on of themselves, just as your heart beats, or as your muscles of breathing continue to act. But this is an acquired habit;

this is what we call a "secondarily automatic" action. Now that phrase is not very difficult when you understand it. By automatic we mean an action taking place of itself. I daresay most of you have seen automata of one kind or another, such as children's toys and more elaborate pieces of mechanism, which, being wound up with a spring, and containing a complicated series of wheels and levers, execute a variety of movements. In each of the Great Exhibitions there have been very curious automata of this kind. We speak then of the actions being "automatic," when we mean that they take place of themselves, without any direction on our own parts; such as the act of sucking in the infant, the acts of respiration and swallowing, and others which are entirely involuntary, and are of this purely reflex character. Now those are "primarily automatic," that is originally automatic; we are born with a tendency to execute them; but the actions of the class I am now speaking of are executed by the same portion of the nervous system—the spinal cord—and are "secondarily automatic," that is to say, we have to learn them, but when once learned, they come very much into the condition of the others, only we have some power of will over them. We start ourselves in the morning by an act of the will; we are determined to go to a particular place; and it may be that we are conscious of these movements over the whole of our walk; but, on the other hand, we may be utterly unconscious of them, and continue to be so until either we have arrived at our journey's end or begin to feel fatigued. Now when we begin to feel fatigued, we are obliged to maintain the action by an effort of the will; we are no longer unconscious, and we are obliged to struggle against the feeling of fatigue, to exert our muscles in order to continue the action.

Now, having set before you this reflex action of the Spinal Cord, you will ask me perhaps what is the exciting cause of this succession of actions in walking. I believe it is the contact of the ground with the foot at each movement. We put down the foot, that suggests as it were to the spinal cord the next movement of the leg in advance, and that foot comes down in its turn, and so we follow with this regular rhythmical succession of movements.

We next pass to a set of centres somewhat higher, those which form the summit, as it were, of this spinal cord, which are really imbedded in the brain, but which do not form a part of that higher organ, which is in fact the organ of the higher part of our mental nature, yet which are commonly included in that which we designate the brain. In fact, the anatomist who only studies the

human brain is very liable to be misled in regard to the character of these different parts, by the fact that the higher part—that which we call the Cerebrum—is so immensely developed in Man, in proportion to the rest of the animal creation, that it envelopes, as it were, the portion of which I am about to speak, concealing it and reducing it apparently to the condition of a very subordinate part; and yet that subordinate part is, as I shall show you, the foundation or basis of the higher portion—the Cerebrum itself. The brain of a Fish consists of very little else than a series of these ganglia, these little knots—the word "ganglion" means "knot," and the ganglia in many instances, when separated, are little knots, as it were, upon the nerves. The brain of a fish consists of a series of these ganglia, one pair belonging to each principal organ of sense. Thus we have in front the ganglia of smell, then the ganglia of sight, the ganglia of hearing, and the ganglia of general sensation. These constitute almost entirely the brain of the fish. There is scarcely anything in the brain of the fish which answers to the Cerebrum or higher part of the brain of man. I will give you an idea of the relative development of these parts. [Dr. Carpenter made other sketches on the black board to represent these ganglia of sense in man and the lower animals.] Now, the Cerebrum in most fishes is a mere little film, overlaying the sensory tract, but in the higher fish we have it larger; in the reptiles we have it larger still; and in birds we have it still larger; in the lower mammalia it is larger still; and then as we ascend to man this part becomes so large in proportion that my board will not take it in. This Cerebrum, this great mass of the brain, at the bottom of which these Ganglia of Sense are buried, as it were, so overlies and conceals them that their essential functions for a long time remained unknown. Now, in the Cerebrum, the position of the active portion, what we call the ganglionic matter, that which gives activity and power to these nervous centres, is peculiar. In all ganglia this "grey" matter, as it is called, is distinct from the white matter. In ordinary ganglia, this grey matter lies in the interior as a sort of little kernel; but in the Cerebrum it is spread out over the surface, and forms a film or layer. If any of you have the curiosity to see what it is like, you have only to get a sheep's brain and examine it, and you will see this film of a reddish substance covering the surface of the Cerebrum. In the higher animals and in man this film is deeply folded upon itself, with the effect of giving it a very much more extended surface, and in this manner the blood vessels come into relation with it; and it is by the changes which take place between

this nervous matter and the blood that all our nervous power is produced. You might liken it roughly to the galvanic battery by which the electric telegraph acts, the white or fibrous portion of the brain and nerves being like the conducting wires of the telegraph. Just as the fibres of the nerves establish a communication between the organs of sensation and the ganglionic centres, and again between the ganglionic centres and the muscles, so do the white fibres which form a great part of the brain, establish a communication between the grey matter of the convoluted or folded surface of the Cerebrum and the Sensory Ganglia at its base. Now I believe that this sensory tract which lies at the base of the skull is the real *Sensorium*, that is, the centre of sensation; that the brain at large, the cerebrum, the great mass of which I have been speaking, is not in itself the centre of sensation; that, in fact, the changes which take place in this grey matter only rise to our consciousness—only call forth our conscious mental activity—when the effect of those changes is transmitted downwards to this Sensorium. Now this Sensorium receives the nerves from the organs of sense. Here, for instance, is the nerve from the organ of smell, here from the eye, and here from the body generally (the nerves of touch), and here the nerves of hearing—every one of these has its own particular function. Now these Sensory ganglia have in like matter reflex actions. I will give you a very curious illustration of one of these reflex actions. You all know the start we make at a loud sound or a flash of light; the stimulus conveyed through our eyes from the optic nerve to the central ganglion, causing it to send through the motor nerves a mandate that calls our muscles into action. Now this may act sometimes in a very important manner for our protection, or for the protection of some of our delicate organs. A very eminent chemist a few years ago was making an experiment upon some extremely explosive compound which he had discovered. He had a small quantity of this compound in a bottle, and was holding it up to the light, looking at it intently; and whether it was a shake of the bottle or the warmth of his hand, I do not know, but it exploded in his hand, the bottle was shivered into a million of minute fragments, and those fragments were driven in every direction. His first impression was that they had penetrated his eyes, but to his intense relief he found presently that they had only penetrated the outside of his eyelids. You may conceive how infinitesimally short the interval was between the explosion of the bottle and the particles reaching his eyes; and yet in that interval the impression had been made upon his sight, the mandate of the reflex action,

so to speak, had gone forth, the muscles of his eyelids had been called into action, and he had closed his eyelids before the particles reached them, and in this manner his eyes were saved. You see what a wonderful proof this is of the way in which the automatic action of our nervous apparatus enters into the sustenance of our lives, and the protection of our most important organs from injury.

Now I have to speak of the way in which this Automatic action of the Sensory nerves and of the motor nerves which answer to them, grows up as it were in ourselves. We will take this illustration. Certain things are originally instinctive, the tendency to them is born with us; but in a very large number of things we educate ourselves, or we are educated. Take, for instance, the guidance of the class of movements I was speaking of just now—our movements of locomotion. We find that when we set off in the morning with the intention of going to our place of employment, not only do our legs move without our consciousness, if we are attending to something entirely different, but we guide ourselves in our walk through the streets; we do not run up against anybody we meet; we do not strike ourselves against the lamp posts; and we take the appropiate turns which are habitual to us. It has often happened to myself, and I dare say it has happened to every one of you, that you have intended to go somewhere else—that when you started you intended instead of going in the direct line to which you were daily accustomed, to go a little out of your way to perform some little commission; but you have got into a train of thought and forgotten yourself, and you find that you are half way along your accustomed track before you become aware of it. Now there you see is the same automatic action of these sensory ganglia—we see, we hear—for instance, we hear the rumbling of the carriages, and we avoid them without thinking of it—our muscles act in respondence to these sights and sounds—and yet all this is done without our intentional direction—they do it for us. Here again, then, we have the "secondarily automatic" action of this power, that of a higher nervous apparatus which has grown, so to speak, to the mode in which it is habitually exercised. Now that is a most important consideration. It has grown to the mode in which it is habitually exercised; and that principle, as we shall see, we shall carry into the higher class of Mental operations.

But there is one particular kind of this action of the Sensory nerves to which I would direct your attention, because it leads us

to another very important principle. You are all of you, I suppose, acquainted with the action of the Stereoscope; though you may not all know that its peculiar action, the perception of solidity it conveys to us, depends upon the combination of two dissimilar pictures—the two dissimilar pictures which we should receive by our two eyes of an object if it were actually placed before us. If I hold up this jug for instance before my eyes, straight before the centre of my face, my two eyes receive pictures which are really dissimilar. If I made two drawings of the jug, first as I see it with one eye and, then with the other, I should represent this object differently. For instance, as seen with the right eye I see no space between the handle and the body of the jug; as I see it with the left eye I see a space there. If I were to make two drawings of that jug as I now see it with my two eyes, and put them into a stereoscope, they would bring out, even if only in outline, the conception of the solid figure of that jug in a way that no single drawing could do. Now that conception is the result of our early acquired habit of combining with that which we *see* that which we *feel*. That habit is acquired during the first twelve or eighteen months of infancy. When your little children are lying in their cradles and are handling a solid object, a block of wood, or a simple toy, and are holding it at a distance from their eyes, bringing it to their mouth and then carrying it to arm's length, they are going through a most important part of their education; that part of their education which consists in the harmonization of the mental impressions derived from sight and those derived from the touch; and it is by that harmonization that we get that conception of solidity or projection, which, when we have once acquired it we receive from the combination of these two dissimilar pictures alone, or even, in the case of objects familiar to us, without two dissimilar pictures at all—the sight of the object suggesting to us the conception of its solidity and of its projection.

Now this is a thing so familiar to you, that few of you have probably ever thought of reasoning it out; and in fact it has only been by the occurrence of cases in which persons have grown to adult age without having acquired this power, from having been born blind and having only received sight by a surgical operation at a comparatively late period, when they could describe things as they saw them—I say it is only by such cases that we have come to know how completely dissimilar and separate these two classes of impressions really are, and how important is this process of early infantile education of which I have spoken. A case occurred a few years ago in London where a friend of my

own performed an operation upon a young woman who had been born blind, and though an attempt had been made in early years to cure her, that attempt had failed. She was able just to distinguish large objects, the general shadow as it were of large objects without any distinct perception of form, and to distinguish light from darkness. She could work well with her needle by the touch, and could use her scissors and bodkin and other implements by the training of her hand, so to speak, alone. Well, my friend happened to see her, and he examined her eyes, and told her that he thought he could get her sight restored; at any rate, it was worth a trial. The operation succeeded; and being a man of intelligence and quite aware of the interest of such a case, he carefully studied and observed it; and he completely confirmed all that had been previously laid down by the experience of similar cases. There was one little incident which will give you an idea of the education which is required for what you would suppose is a thing perfectly simple and obvious. She could not distinguish by sight the things that she was perfectly familiar with by the touch, at least, when they were first presented to her eyes. She could not recognise even a pair of scissors. Now you would have supposed that a pair of scissors, of all things in the world, having been continually used by her, and their form having become perfectly familiar to her hands, would have been most readily recognised by her sight; and yet she did not know what they were; she had not an idea until she was told, and then she laughed, as she said, at her own stupidity. No stupidity at all; she had never learned it, and it was one of those things which she could not know without learning. One of the earliest cases of this kind was related by the celebrated Cheselden, a surgeon of the early part of last century. Cheselden relates how a youth just in this condition had been accustomed to play with a cat and a dog; but for some time after he attained his sight he never could tell which was which, and used to be continually making mistakes. One day being rather ashamed of himself for having called the cat the dog, he took up the cat in his arms and looked at her very attentively for some time, stroking her all the while; and in this way he associated the impression derived from the sight of the cat with the impression derived from the touch, and made himself master (so to speak) of the whole idea of the animal. He then put the cat down, saying, "Now puss, I shall know you another time."

Now, the reason why I have specially directed your attention to this is because it leads to one of the most important principles

that I desire to expound to you this evening—what I call in Mental Physiology the doctrine of *resultants*. All of you who have studied merchanics know very well what a "resultant" means. You know that when a body is acted on by two forces at the same time, one force carrying it in this direction, and another force in that direction, we want to know in what direction it will go, and how far it will go. To arrive at this we simply complete what is called the parallelogram of forces. In fact it is just as if a body was acted on at two different times, by a force driving it in one direction, and then by a force driving it in the other direction [Dr. Carpenter illustrated this point by the aid of the blackboard.] We draw two lines parallel to this, and we draw a diagonal—that diagonal is what is called the resultant; that is, it expresses the direction, and it expresses the distance—the length of the motion which that body will go when acted upon by these two forces. Now I use this term as a very convenient one to express this—that when we have once got the conception that is derived from the harmonisation of these two distinct sets of impressions on our nerves of sense, we do not fall back on the original impressions, but we fall back on the resultant, so to speak. The thing has been done for us; it is settled for us; we have got the resultant; and the combination giving that resultant is that which governs the impression made upon our minds by all similar and future operations of the same kind. We do not need to go over the processes of judgment by which the two sets of impressions are combined in every individual case; but we fall back, as it were, upon the resultant. Now what is the case in the harmonisation of the two classes of impressions of sight and touch, I believe to be true of the far more complicated operations of the mind of which the higher portion of the brain, the Cerebrum, is the instrument. Now this Cerebrum we regard as furnishing, so to speak, the mechanism of our thoughts. I do not say that the Cerebrum is that which does the whole work of thinking, but it furnishes the mechanism of our thought. It is not the steam engine that does the work; the steam engine is the mere mechanism; the work is done, as my friend Professor Roscoe would tell you, by the heat supplied; and if we go back to the source of that heat, we find it originally in the heat and light of the sun that made the trees grow by which the coal was produced, in which the heat of the sun is stored up, as it were, and which we are now using, I am afraid, in rather wasteful profusion. The steam engine furnishes the mechanism; the work

is done by the force. Now in the same manner the brain serves as the mechanism of our thought; and it is only in that sense that I speak of the work of the brain. But there can be no question at all that it works of itself, as it were,—that it has an automatic power, just in the same manner as the sensory centres and the spinal cord have automatic power of their own. And that a very large part of our mental activity consists of this automatic action of the brain, according to the mode in which we have trained it to action, I think there can be no doubt whatever. And the illustration with which I started in this lecture gives you, I believe, a very good example of it. However, there are other examples which are in some respects still better illustrations of the automatic work that is done by the brain, in the state which is sometimes called Second Consciousness, or Somnambulism—to which some persons are peculiarly subject. I heard only a few weeks ago of an extremely remarkable example of a young man who had overworked himself in studying for an examination, and who had two distinct lives, as it were, in each of which his mind worked quite separately and distinct from the other. One of these states, however,—the ordinary one—is under the control of the will to a much greater extent than the other; while the secondary state is purely, I suppose, automatic. There are a great many instances on record of very curious mental work, so to speak, done in this automatic condition—a state of active dreaming in fact. For instance, Dr. Abercrombie mentions, in his very useful work on the Intellectual Powers, an example of a lawyer who had been excessively perplexed about a very complicated question. An opinion was required from him, but the question was one of such difficulty that he felt very uncertain how his opinion should be given. The opinion had to be given on a certain day, and he awoke in the morning of that day with a feeling of great distress. He said to his wife, "I had a dream, and the whole thing in that dream has been clear before my mind, and I would give anything to recover that train of thought." His wife said to him, "Go and look on your table." She had seen him get up in the night and go to his table and sit down and write. He went to his table, and found there the very opinion which he had been most earnestly endeavouring to recover, lying in his own handwriting. There was no doubt about it whatever, and this opinion he at once saw was the very thing which he had been anxious to be able to give. A case was put on record of a very similar kind only a few years ago by a gentleman well known in

London, the Rev. John De Liefde, a Dutch clergyman. This gentleman mentioned it on the authority of a fellow student who had been at the college at which he studied in early life. He had been attending a class in mathematics, and the professor said to his class one day—"A question of great difficulty has been referred to me by a banker, a very complicated question of accounts—which they have not themselves been able to bring to a satisfactory issue, and they have asked my assistance. I have been trying, and I cannot resolve it. I have covered whole sheets of paper with calculations, and have not been able to make it out. Will you try?" He gave it as a sort of problem to his class, and said he should be extremely obliged to any one who would bring him the solution by a certain day. This gentleman tried it over and over again; he covered many slates with figures, but could not succeed in resolving it. He was a little put on his mettle, and very much desired to attain the solution; but he went to bed on the night before the solution, if attained, was to be given in, without having succeeded. In the morning, when he went to his desk, he found the whole problem worked out in his own hand. He was perfectly satisfied that it was his own hand; and this was a very curious part of it—that the result was correctly obtained by a process very much shorter than any he had tried. He had covered three or four sheets of paper in his attempts, and this was all worked out upon one page, and correctly worked, as the result proved. He inquired of his "hospita," as she was called—I believe our English equivalent is bedmaker, the woman who attended to his rooms—and she said she was certain that no one had entered his room during the night. It was perfectly clear that this had been worked out by himself.

Now there are many cases of this kind, in which the mind has obviously worked more clearly and more successfully in this automatic condition, when left entirely to itself, than when we have been cudgelling our brains, so to speak, to get the solution. I have paid a good deal of attention to this subject, in this way:—I have taken every opportunity that occurred to me of asking inventors and artists—creators in various departments of art—musicians, poets, and painters, what their experience has been in regard to difficulties which they have felt, and which they have after a time overcome. And the experience has been almost always the same, that they have set the result which they have wished to obtain strongly before their minds, just as we do when we try to recollect something we have forgotten; they think of

everything that can lead to it; but if they do not succeed, they put it by for a time, and give their minds to something else, and endeavour to obtain as complete a repose or refreshment of the mind upon some other occupation as they can; and they find that either after sleep, or after some period of recreation by a variety of employment, just what they want comes into their heads. A very curious example of this was mentioned to me a few years ago by Mr. Wenham, a gentleman who has devoted a great deal of time and attention to the improvement of the microscope, and who is the inventor of that form of binocular microscope (by which we look with two eyes and obtain a stereoscopic picture), which is in general use in this country. The original binocular microscope was made upon a plan which would suggest itself to any optician. I shall not attempt to describe it to you, but it involved the use of three prisms, giving a number of reflections; and every one of these reflections was attended with a certain loss of light and a certain liability to error. And beside that, the instrument could only be used as a binocular microscope. Now Mr. Wenham thought it might be possible to construct an instrument which would work with only one prism, and that this prism could be withdrawn, and then we could use the microscope for purposes to which the binocular microscope could not be applied. He thought of this a great deal, but he could not think of the form of prism which would do what was required. He was going into business as an engineer, and he put his microscopic studies aside for more than a fortnight, attending only to his other work, and thinking nothing of his microscope. One evening after his day's work was done, and while he was reading a stupid novel, as he assured me, and was thinking nothing whatever of his microscope, the form of the prism that should do this work flashed into his mind. He fetched his mathematical instruments, drew a diagram of it, worked out the angles which would be required, and the next morning he made his prism, and found it answered perfectly well; and upon that invention nearly all the binocular microscopes made in this country have since been constructed.

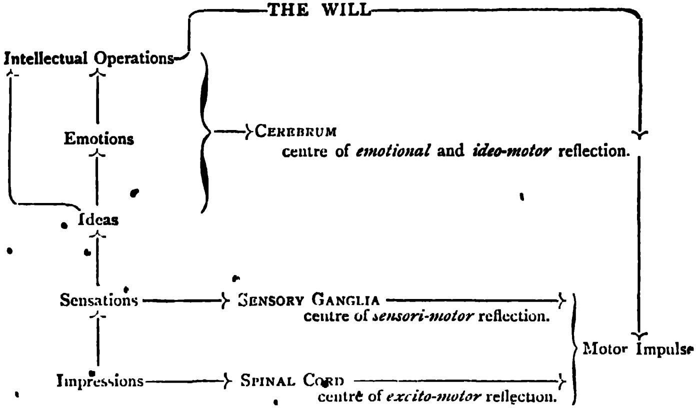
I could tell you a mumber of anecdotes of this kind which would show you how very important is this automatic working of our minds—this work which goes on without any more control or direction of the Will, than when we are walking and engaged in a train of thought which makes us unconscious of the movements of our legs. And I believe that in all these instances—such as those I have named, and a long series of others—the result is owing to

the mind being left to itself without the disturbance of any emotion. It was the worry which the bank manager had been going through, that really prevented the mind from working with the steadiness and evenness that produced the result. So in the case of the lawyer; so in the case of the mathematician; they were all worrying themselves, and did not let their minds have fair play. You have heard, I dare say, and those of you who are horsemen may have had experience, that it is a very good thing sometimes, if you lose your way on horseback, to drop the reins on the horse's back and let him find his way home. You have been guiding the horse into one path and into another, and following this and that path, and you find that it does not lead you in the right direction; just let the horse go by himself, and he will find his way better than you can. In the same manner, I believe, that our minds, under the circumstances I have mentioned, really do the work better than our wills can direct. The will gives the impulse in the first instance, just as when you start on your walk; and not only this, but the will keeps before the mind all the thoughts which it can immediately lay hold of, or which association suggests, that bear upon the subject. But then these thoughts do not conduct immediately to an issue, they require to work themselves out; and I believe that they work themselves out very often a great deal better by being left to themselves. But then we must recollect that such results as these are only produced in the mind which has been trained and disciplined; and that training and discipline are the result of the control of the Will over the mental processes, just as in the early part of the lecture I spoke to you of the act of speech as made possible by the control which the will has over the muscles of breathing. We cannot stop these movements—we must breathe—but we can regulate them, and modify them, and intensify them, or we can check them for a moment, in accordance with the necessities of speech. Well, so it is, I think, with regard to the action of our will upon our mental processes. I believe that this control, this discipline of the will, should be learned very early; and I will give to the mothers amongst you, especially, one hint in regard to a most valuable mode of training it even in early childhood. I learned this, I may say, from a nurse whom I was fortunate enough to have, and whose training of my own sons in early childhood I regard as one of the most valuable parts of their education. She was a sensible country girl, who could not have told her reasons, but whose instincts guided her in the right direc-

tion. I studied her mode of dealing with the children, and learned from that the principle. Now the principle is this:—A child falls down and hurts itself. (I take the most common of nursery incidents. You know that Sir Robert Peel used to say that there were three ways of looking at this question; and there are three modes of dealing with this commonest of nursery incidents.) One nurse will scold the child for crying. The child feels the injustice of this; it feels the hurt, and it feels the injustice of being scolded. I believe that is the most pernicious of all the modes of dealing with it. Another coddles the child, takes it up and rubs its head, and says, "O naughty chair, for hurting my dear child!" I remember learning that one of the royal children fell against a table in the Queen's presence, and the nurse said, "O naughty table," when the Queen very sensibly said, "I will not have that expression used; it was not the table that was naughty; it was the child's fault that he fell against the table." I believe that this method is extremely injurious; the result of it being that it fixes the child's attention upon its hurt, and causes it to attain that habit of self-consciousness which is in after life found to have most pernicious effects. Now, what does the sensible and judicious nurse do? She distracts the child's attention, holding it up to the window to look at the pretty horses, or gets it a toy to look at. This excites the child's attention, and the child forgets its hurt, and in a few moments is itself again, unless the hurt has been severe. When I speak of coddling, I mean about a trifling hurt such as is forgotten in a few moments; a severe injury is a different matter. But I believe that the coddling is only next in its evil results (when followed out as a system) to the evil effects of the system of scolding; the distraction of the attention is the object to be aimed at. Well, after a time the child comes to be able to distract its own attention. It feels that it can withdraw its own mind from the sense of its pain, and can give its mind to some other object, to a picture-book or to some toy, or whatever the child feels an interest in; and that is the great secret of self-government in later life. We should not say, "I wont think of of this"—some temptation, for instance; *that* simply fixes the attention upon the very thought that we wish to escape from; but the true method is—"I will think of something else;" *that*, I believe, is the great secret of self-government, the knowledge of which is laid in the earliest periods of nursery life.

Now just direct your attention to this diagram, as a sort of summary of the whole:—

[Diagram.]



You see I put at the top the Will. The will dominates everything else. I do not pretend to explain it, but I simply say, as Archbishop Manning said, in applying my own language to this case, that our common sense teaches us that we have a will, that we have the power of self-government and self-direction, and that we have the power of regulating and dominating all these lower tendencies to a certain extent, not to an unlimited extent. We cannot prevent those thoughts and feelings rising in our minds that we know to be undesirable; but we can escape from them, we can repress them; but as I said the effort to escape from them is much more effectual than the effort to repress them, excepting when they arise with great power, and then we have immediately, as it were, to crush them out; but when they tend to return over and over again, the real mode of subduing them is to determine to give our attention to something else. It is by this exercise of the will, therefore, in training and disciplining the mind, that it acquires that method by which it will work of itself. The mathematician could never have worked out that difficult problem, nor the lawyer have given his opinion, nor the artist have developed those conceptions of beauty which he endeavours to shape either in music, or poetry, or painting, but for the training and disciplining which his mind has undergone. The most wonderfully creative of all musicians, Mozart, whose music flowed from him with a spontaneousness that no musician, I think, has ever equalled—Mozart went through, in early life, a most elaborate course of

study, imposed upon him, in the first instance, by his father, and afterwards maintained by himself. When his cotemporaries remarked how easily his compositions flowed from him, he replied, "I gained the power by nothing but hard work." Mozart had a most extraordinary combination of this intuitive musical power, with the knowledge derived from patient and careful study, that probably any man ever attained. Now in the same manner we have persons of extraordinary natural gifts, and see these gifts frequently running to waste, as it were, because they have not received this culture and discipline. And it is this discipline which gives us the power of performing, unconsciously to ourselves, these elaborate mental operations; because I hold that a very large part of our mental life thus goes on, not only automatically, but even below the sphere of our consciousness. And you may easily understand this if you refer to the diagram which I drew just now on the blackboard. You saw, that the Cerebrum, the part that does the work, what is called the convoluted surface of the brain, lies just immediately under the skull cap; that it is connected with the sensorium at the base of the brain by a series of fibres which are merely, I believe, conducting fibres. Now I think that it is just as possible that the Cerebrum should work by itself when the sensorium is otherwise engaged or in a state of unconsciousness, as that impressions should be made on the eye of which we are unconscious. A person may be sleeping profoundly, and you may go and raise the lid and bring a candle near, and you will see the pupil contract; and yet that individual shall see nothing, for he is in a state of perfect unconsciousness. His eye sees it, so to speak, but his mind does not; and you know that his eye sees it by the contraction of the pupil, which is a reflex action; but his mind does not see it, because the sensorium is in a state of inaction. In the same manner during sleep the Cerebrum may be awake and working, and yet the Sensorium shall be asleep, and we may know nothing of what the cerebrum is doing except by the results. And it is in this manner, I believe, that, having been once set going, and the cerebrum having been shaped, so to speak, in accordance with our ordinary processes of mental activity, having grown to the kind of work we are accustomed to set it to execute, the cerebrum can go on and do its work for itself. The work of invention, I am certain, is so mainly produced, from concurrent testimony I have received from a great number of inventors, or what the old English called "makers"—what the Greeks called poets, because the word poet means a maker.

Every inventor must have a certain amount of imagination, which may be exercised in mechanical contrivance or in the creations of art; these are *inventions*—they are made, they are produced, we don't know how; the conception comes into the mind we cannot tell whence; but these inventions are the result of the original capacity for that particular kind of work, trained and disciplined by the culture we have gone through. It is not given to every one of us to be an inventor. We may love art thoroughly, and yet we may never be able to evolve it for ourselves. So in regard to humour. For instance, there are some men who throw out flashes of wit and humour in their conversation, who cannot help it—it flows from them spontaneously. There are other men who enjoy this amazingly, whose nature it is to relish such expressions keenly, but who cannot make them themselves. The power of invention is something quite distinct from the intellectual capacity or the emotional capacity for enjoying and appreciating; but although we may not have these powers of invention, we can all train and discipline our minds to utilise that which we do possess to its utmost extent. And here is the conclusion to which I would lead you in regard to Common Sense. We fall back upon this, that common sense is, so to speak, the general resultant of the whole previous action of our minds. We submit to common sense any questions—such questions as I shall have to bring before you in my next lecture; and the judgment of that common sense is the judgment elaborated as it were by the whole of our mental life. It is just according as our mental life has been good and true and pure, that the value of this acquired and this higher common sense is reached. We may in proportion I believe to our honesty in the search for truth—in proportion as we discard all selfish considerations and look merely at this grand image of truth, so to speak, set before us, with the purpose of steadily pursuing our way toward it—in proportion as we discard all low and sensual feelings in our love of beauty, and especially in proportion to the earnestness of the desire by which our minds are pervaded always to keep the right before us in all our judgments—so I believe will our minds be cleared in their perception of what are merely prudential considerations. It has on several occasions occurred to me to form a decision as to some important change either in my own life, or in the life of members of my family, which involved a great many of what we are accustomed to call *pros* and *cons*—that is, there was a great deal to be said on both sides. I heard the expression once used by a naturalist, with regard to

difficulties in classification,—"It is very easy to deal with the white and the black; but the difficulty is to deal with the grey." And so it is in life. It is perfectly easy to deal with the white and the black,—there are things which are clearly right, and things which are clearly wrong; there are things which are clearly prudent, and things which are clearly imprudent; but a great many cases arise in which even right and wrong may seem balanced, or the motives may be so balanced that it is difficult to say what is right; and again there are cases in which it is difficult to say what is prudent; and I believe in these cases where we are not hurried and pressed for a decision, the best plan is to do exactly that which I spoke of in the earlier part of the lecture—to set before us as much as possible everything that is to be said on both sides. Let us consider this well; let us go to our friends; let us ask what they think about it. They will suggest considerations which may not occur to ourselves. It has happened to me within the last three or four months to have to make a very important decision of this kind for myself; and I took this method—I heard everything that was to be said on both sides, I considered it well, and then I determined to put it aside as completely as possible for a month, or longer, if time should be given, and then to take it up again, and simply just to see how my mind gravitated—how the balance then turned. And I assure you that I believe that in those who have disciplined their minds in the manner I have mentioned, that act of "Unconscious Cerebration," for so I call it, this unconscious operation of the brain in balancing for itself all these considerations, in putting all in order, so to speak, in working out the result—I believe that that process is far more likely to lead us to good and true results than any continual discussion and argumentation, in which one thing is pressed with undue force and then that leads us to bring up something on the other side, so that we are just driven into antagonism, so to speak, by the undue pressure of the force which we think is being exerted. I believe that to hear everything that is to be said, and then not to ruminate upon it too long, not to be continually thinking about it, but to put it aside entirely from our minds as far as we possibly can, is the very best mode of arriving at a correct conclusion. And this conclusion will be the *resultant* of the whole previous training and discipline of our minds. If that training and discipline has all been in the direction of the true and the good, I believe that we are more likely to obtain a valuable result from such a process than from any conscious discussion of it in our minds, anything like continually bringing it

up and thinking of it, and going over the whole subject again in our thoughts. The unconscious settling down, as it were, of all these respective motives, will I think incline the mind ultimately to that which is the just and true decision.

There is just one other point I could mention in connection with this subject: the manner in which the *conscious* direction and discipline of the mind will tend to remove those *unconscious* prejudices that we all have more or less from education, from the circumstances in which we were brought up; and from which it is excessively difficult for us to free ourselves entirely. I have known a great many instances in public and in private life, in which the most right-minded men have every now and then shown the trammelling, as it were, of their early education and early associations, and were not able to think clearly upon the subject in consequence of this. These early prejudices and associations cling around us and influence the thoughts and feelings of the honestest men in the world unconsciously; and it is sometimes surprising to those who do not know the force of these early associations, to see how differently matters which are to them perfectly plain and obvious are viewed by men whom we feel we must respect and esteem. Now I believe that it is the earnest habit of looking at a subject from first principles, and, as I have said over and over again, looking honestly and steadily at the true and the right, which gives the mind that direction that ultimately overcomes the force of these early prejudices and these early associations, and brings us into that condition which approaches the nearest of anything that I think we have the opportunity of witnessing in our earthly life, to that *direct insight*, which many of us believe will be the condition of our minds in that future state in which they are released from all the trammels of our corporeal existence.

EPIDEMIC DELUSIONS.

A LECTURE

BY DR. CARPENTER, F.R.S.,

Delivered in the Hulme Town Hall, Manchester, December 8th, 1871.

OUR subject to-night links itself in such a very decided manner to the subject in which we were engaged last week, and the illustrations which I shall give you are so satisfactorily explained on the scientific principle which I endeavoured then to expound to you, that I would spend a very few minutes in just going over some of the points to which I then particularly directed your attention. My object was to show you that between our Mental operations and our Will there is something of that kind of relation which exists between a well-trained horse and his rider; that the will,—if rightly exercised in early infancy in directing and controlling the mental operations; in directing the attention to the objects to which the intellect should be applied; in controlling and repressing emotional disturbance; restraining the feelings when unduly excited, and putting a check upon the passions—that the will in that respect has the same kind of influence over the mind, or ought to have, as the rider has upon his horse; that the powers and activities of the mind are to a very great degree independent of the will; that the mind will go on of itself without any more than just the starting of the will, in the same manner as a horse will go on in the direction that it has been accustomed to go with merely the smallest impulse given by the voice, or the hand, or the heel of the rider, and every now and then a very slight check (if it is a well-trained horse) or guidance from the bridle or from a touch of the spur, and will follow exactly the course that the rider desires, but by its own independent power. And, again, I

showed you that as there are occasions on which a horse is best left to itself, so there are occasions when the mind is best left to itself, without the direction and control of the will; in fact in which the operations of the mind are really disturbed by being continually checked and guided and pulled up by the action of the will, the result being really less satisfactory than when the mind, previously trained and disciplined in that particular course of activity, is left to itself. I gave you some curious illustrations of this from occurrences which have taken place in Dreaming, or in that form of dreaming which we call Somnambulism: where a legal opinion had been given, or a mathematical problem had been resolved, in the state of sleep waking; that is to say the mind being very much in the condition of that of the dreamer, its action being altogether automatic, going on of itself without any direction or control from the will —but the bodily activity obeying the direction of the mind. And then I went on to show you that this activity very often takes place, and works out most important results, even without our being conscious of any operations going on; and that some of these results are the best and most valuable to us in bringing at last to our consciousness ideas which we have been vainly searching for,—as in the case where we have endeavoured to remember something that we have not at first been able to retrace, and which has flashed into our minds in a few hours, or it may be a day or two afterwards; or, again, when we have been directing our minds to the solution of some problem which we have put aside in a sort of despair, and yet in the course of a little time that solution has presented itself while our minds have either been entirely inactive, as in sleep, or have been directed into some entirely different channel of action.

Now, like the well-trained horse which will go on of itself with the smallest possible guidance, yet still under the complete domination of the rider, and will even find its way home when the rider cannot direct it thither, we find that the human mind sometimes does that which even a well-trained horse will do—that it runs away from the guidance of its directing will. Something startles the horse, something gives it alarm; and it makes a sudden bound, and then, perhaps, sets off at a gallop, and the rider cannot pull it up. This alarm often spreads contagiously, as it were, from one horse to another; as we lately saw in the "stampede" at Aldershot. Or, again, a horse, even if well trained, when he gets a new rider, sometimes, as we say, "tries it on," to see whether the horse or the rider is really the master. I have heard many horsemen say that that is a very

familiar experience. When you first go out with a new horse, it may be to a certain degree restive; but if the horse finds that you keep a tight hand upon him, and that his master knows well how to keep him under control, a little struggling may have to be gone through, and the horse from that time becomes perfectly docile and obedient. But if, on the other hand, the horse finds that he is the master, even for a short time, no end of trouble is given afterwards to the rider in acquiring that power which he desires to possess. Now that is just the case with our minds; we may follow out the parallel very closely indeed. We find that if our minds once acquire habits—habits of thought, habits of feeling—which are independent of the will, which the will has not kept under adequate regulation, these habits get the better of us; and then we find that it is very difficult indeed to recover that power of self direction which we have been aiming at, and which the well-trained and well-disciplined mind will make its highest object. So, again, we find that there are states in which, from some defect in the physical condition of the body, or it may be from some great shock which has affected the mind and weakened for a time the power of the will, very slight impulses—just like the slight things that will make a horse shy—will disturb us unduly; and we feel that our emotions are excited in a way that we cannot account for, and we wonder why such a little thing should worry and vex us in the way that it does. Even the best of us know, within our own personal experience, that when we are excessively fatigued in body, or overstrained in mind, our power of self-control is very much weakened; so that particular ideas will take possession of us, and for a time will guide our whole course of thought, in a manner which our sober judgment makes us feel to be very undesirable. What, for instance, is more common than for a person to take offence at something that has been said or done by his most intimate friend, or by some member of his family; merely because he has been jaded or overtasked, and has not the power of bringing to the fair judgment of his common sense the question whether that offence was really intended, or whether it was a thing he ought not to take any notice of? He broods over this notion, and allows it to influence his judgment; and if he does not in a day or two rouse himself and master his feelings by throwing it off, it may give rise to a permanent estrangement. We are all of us conscious of states of mind of that kind.

But there are states of mind which lead to very much more serious disorder, arising from the neglect of that primary dis

cipline and culture on which I have laid so much stress. We find that ignorance, and that want of the habit of self-control which very commonly accompanies it, predispose very greatly indeed to the violent excitement of the feelings, and to the possession of the mind by ideas which we regard as essentially absurd; and under these states of excitement of feeling, and the tendency of these dominant ideas to acquire possession of the intellect, the strangest aberrations take place, not only in individuals but in communities; and it is of such that I have especially to speak to-night. We know perfectly well, in our individual experience, that these states tend to produce Insanity if they are indulged in, and if the individual does not make an earnest effort to free himself from their influence. But, looking back at the history of the earlier ages, and carrying that survey down to the present time, we have experience in all ages of great masses of people being seized upon by these dominant ideas, accompanied with the excitement of some passion or strong impulse which leads to the most absurd results; and it is of these Epidemic Delusions I have to now speak. The word "epidemic" simply means something that falls upon, as it were, the great mass of the people—a delusion which affects the popular mind. And I believe that I can best introduce the subject to you by showing you how, in certain merely physical conditions, mere bodily states, there is a tendency to the propagation, by what is commonly called imitation, of very strange actions of the nervous system. I suppose there is no one of you who does not know what an hysteric fit means; a kind of fit to which young women are especially subject, but which affects the male sex also. One reason why young women are particularly subject to it is that in the female the feelings are more easily excited, while the male generally has a less mobile nervous system, his feelings being less easily moved, while he is more influenced by the intellect. These hysteric fits are generally brought on by something that strongly affects the feelings. Now, it often happens that a case of this sort presents itself in a school or nunnery, sometimes in a factory where a number of young women are collected together; one being seized with a fit, others will go off in a fit of a very similar kind. There was an instance a good many years ago in a factory in a country town in Lancashire, in which a young girl was attacked with a violent convulsive fit, brought on by alarm, consequent upon one of her companions, a factory operative, putting a mouse down inside her dress. The girl had a particular antipathy to mice, and the sudden shock threw her into a violent fit. Some of

the other girls who were near very soon passed off into a similar fit; and then there got to be a notion that these fits were produced by some emanations from a bale of cotton; and the consequence was that they spread, till scores of the young women were attacked day after day with these violent fits. The medical man who was called in saw at once what the state of things was; he assured them in the first place that this was all nonsense about the cotton; and he brought a remedy, in the second place, which was a very appropriate one under the circumstances—namely, an electrical machine; and he gave them some good violent shocks, which would do them no harm, assuring them that this would cure them. And cure them it did. There was not another attack afterwards.—I remember very well that when I was a student at Bristol, there was a ward in the hospital to which it was usual to send young servant girls; for it was thought undesirable that these girls should be placed in the ward with women of a much lower class, especially the lower class of Irish women who inhabited one quarter of Bristol, as I believe there is an Irish quarter in Manchester. These girls were mostly respectable, well-conducted girls, and it was thought better that they should be kept together. Now the result of this was that if an hysteric fit took any one of them, the others would follow suit; and I remember perfectly well, when I happened to be a resident pupil, having to go and scold these girls well, threatening them with some very severe infliction. I forget what was threatened; perhaps it would be a shower bath, for anyone who went off into one of these fits. Now here the cure is effected by a stronger emotion, the emotion of the dread of—we will not call it punishment—but of a curative measure; and this emotion overcame the tendency to what we commonly call imitation. It is the suggestion produced by the sight of one, that brings on the fit in another, where there is the pre-disposition to it.—Now I believe that in all these cases there is something wrong in the general health or in the nervous system; or the suggestion would not produce such results. Take the common teething fits of children. We there see an exciting cause in the cutting of the teeth; the pressure of the tooth against the gum being the immediate cause of the production of convulsive action. But it will not do so in the healthy child. I feel sure that in every case where there is a teething fit, of whatever kind, there is always some unhealthy condition of the nervous system—sometimes from bad food; more commonly from bad air. I have known many instances in which children had fits with every tooth that they cut, yet

when sent into the country they had no recurrence of the fit. There must have been some predisposition, some unhealthy condition of the nervous system, to favour the exciting cause, which, acting upon this predisposition, brings out such very unpleasant results.

There are plenty of stories of this kind that I might relate to you. For instance, in nunneries it is not at all uncommon, from the secluded life, and the attention being fixed upon one subject, one particular set of ideas and feelings—the want of a healthy vent, so to speak, for the mental activity—that some particular odd propensity has developed itself. For instance, in one nunnery abroad, many years ago, one of the youngest nuns began to mew like a cat; and all the others, after a time, did the same. In another nunnery one began to bite, and the others were all affected with the propensity to bite. In one of these instances the mania was spreading like wild-fire through Germany, extending from one nunnery to another; and they were obliged to resort to some such severe measures as I have mentioned to drive it out. It was set down in some instances to demoniacal possession, but the devil was very easily exorcised by some pretty strong threat on the part of the medical man. The celebrated physician, Boerhaave, was called in to a case of that kind in an orphan asylum in Holland, and I think his remedy was a red-hot iron. He heated the poker in the fire, and said that the next girl who fell into one of these fits should be burnt in the arm; this was quite sufficient to stop it. In Scotland at one time there was a great tendency to breaking out into fits of this kind in the churches. This was particularly the case in Shetland; and a very wise minister there told them that the thing could not be permitted, and that the next person who gave way in this manner—as he was quite sure they could control themselves if they pleased—should be taken out and ducked in a pond near. There was no necessity at all to put his threat into execution. Here, you see, the stronger motive is substituted for the weaker one, and the stronger motive is sufficient to induce the individual to put a check upon herself. I have said that it usually happens with the female sex, though sometimes it occurs with young men who have more or less of the same constitutional tendency. What is necessary is to induce a stronger motive, which will call forth the power of self-control which has been previously abandoned.

Now this tendency which here shows itself in convulsive movements of the body, will also show itself in what we may call

convulsive action of the Mind; that is, in the excitement of violent feelings and even passions, leading to the most extraordinary manifestations of different kinds. The early Christians, you know, practised self-mortification to a very great degree; and considered that these penances were so much scored up to the credit side of their account in heaven,—that, in fact, they were earning a title to future salvation by self-mortification. Among other means of self-mortification, they scourged themselves. That was practised by individuals. But in the middle ages this disposition to self-mortification would attack whole communities, especially under the dominant idea that the world was coming to an end. In the middle of the 13th century, about 1250, there was this prevalent idea that the world was coming to an end; and whole communities gave themselves up to this self-mortification by whipping themselves. These Flagellants went about in bands with banners, and even music, carrying scourges; and then, at a given signal, every one would strip off the upper garment (men, women, and children joined these bands), and proceed to flog themselves very severely indeed, or to flog each other. This subsided for a time, but it broke out again during and immediately after that terrible plague which is known as the "black death," which devastated Europe in the reign of Edward III., about the year 1340. This black death seems to have been the Eastern Plague in a very severe form, which we have not known in this country since the great plague of London in Charles II.'s time, and one or two smaller outbreaks since, but which has now entirely left us. The severity of this plague in Europe was so great that upon a very moderate calculation one in four of the entire population were carried off by it; and in some instances it is said that nine-tenths of the people died of it. You may imagine, therefore, what a terrible infliction it was. And you would have supposed that it would have called forth the better feelings of men and women generally; but it did not. One of the worst features, morally, of that terrible affliction, was the lamentable suspension of all natural feelings which it seemed to induce. When any member of a family was attacked by this plague, every one seemed to desert him, or desert her; the sick were left to die alone, or merely under the charge of any persons who thought that they would be paid for rendering this service; and the funerals were carried on merely by these paid hirelings in a manner most repulsive to the feelings: and yet the very people who so deserted their relatives would join the bands of flagellants, who paraded about from place to place, and even from country to country,—mortifying their flesh in this manner for

the purpose of saving their own souls, and, as they said, also making expiation for the great sins which had brought down this terrible visitation. This system of flagellation never gained the same head in this country that it did on the Continent. A band of about 100 came to London about the middle of the reign of Edward III., in the year 1350. They came in the usual style, with banners and even instruments of music, and they paraded the streets of London. At a given signal every one lay down and uncovered the shoulders, excepting the last person, who then flogged every one till he got to the front, where he lay down; and the person last in the rear stood up, and in his turn flogged every one in front of him. Then he went to the front and lay down; and so it went on until the whole number had thus been flogged, each by every one of his fellows. This discipline, however, did not approve itself to the good citizens of London, and it is recorded that the band of flagellants returned without having made any converts. Whether the skins of the London citizens were too tender, or whether their good sense prevailed over this religious enthusiasm, we are not informed; but at any rate the flagellants went back very much as they came, and the system never took root in this country; yet for many years it was carried on elsewhere. One very curious instance is given of the manner in which it fastened on the mind—that mothers actually scourged their new-born infants before they were baptised, believing that in so doing they were making an offering acceptable to God. Now all this appears to us perfectly absurd. We can scarcely imagine the state of mind that should make any sober, rational persons suppose that this could be an offering acceptable to Almighty God; but it was in accordance with the religious ideas of the time: and for a good while even the Church sanctioned and encouraged it, until at last various moral irregularities grew up, of a kind that made the Pope think it a very undesirable thing, and it was then put down by ecclesiastical authority; yet it was still practised in secret for some time longer, so that it is said that even until the beginning of the last century there were small bands of flagellants in Italy, who used to meet for this self-mortification.

That was one form in which a dominant idea took possession of the mind and led to actions which might be called voluntary, for they were done under this impression, that such self-mortification was an acceptable offering. But there were other cases in which the action of the body seemed to be in a very great degree involuntary, just about as involuntary as an hysteric

fit, and yet in which it was performed under a very distinct idea; such was what was called the "Dancing Mania," which followed upon this great plague. This dancing mania seemed in the first instance to seize upon persons who had a tendency to that complaint which we now know as St. Vitus's dance—St. Vitus was in fact the patron saint of these dancers. St. Vitus's dance. or chorea, in the moderate form in which we now know it, is simply this, that there is a tendency to jerking movements of the body, these movements sometimes going on independently of all voluntary action, and sometimes accompanying any attempt at voluntary movement; so that the body of a person may be entirely at rest until he desires to execute some ordinary movement, such as lifting his hand to his head to feed himself, or getting up to walk; then, when the impulse is given to execute a voluntary movement, instead of the muscles obeying the will, the movement is complicated as (it were) with violent jerking actions, which show that there is quite an independent activity. The fact is that stammering is a sort of chorea. We give the name of chorea to this kind of disturbance of the nervous system, and the action of stammering is a limited chorea—chorea limited to the muscles concerned in speech, when the person cannot regulate the muscles so as to bring out the words desired; the very strongest effort of his will cannot make the muscles obey him, but there is a jerking irregular action every time he attempts to pronounce particular syllables. And the discipline that the stammerer has to undergo in order to cure or alleviate his complaint is just the kind of discipline I have spoken of so frequently—the fixing the attention on the object to be gained, and regularly exercising the nerves and muscles in proceeding from that which they *can* do to that which they find a difficulty in doing. That is an illustration of the simpler form of this want of definite control over the muscular apparatus, connected with a certain mental excitement; because everyone knows that a stammerer is very much affected by the condition of his feelings at the time. If, for example, he is at all excited, or if he apprehends that he shall stammer, that is enough to produce it. I have known persons who never stammered in ordinary conversation. yet when in company with stammerers they could scarcely avoid giving way to it; and even when the subject of stammering was talked about, when the idea was conveyed to their minds, they would begin to hesitate and stutter, unless they put a very strong control upon themselves. It is just in this way, then, only in the most exaggerated form, that these persons were afflicted with what was called the dancing mania.

They would allow themselves to be possessed with the idea that they *must* dance; and this dancing went on, bands going from town to town, and taking in any who would join them. Instances are recorded in which they would go on for twenty-four or thirty-six hours, continually jumping and dancing and exerting themselves in the most violent manner, taking no food all this time, until at last they dropped on the ground almost lifeless; and in fact several persons, it is said, did die from pure exhaustion, and this just because they were possessed with the idea that they *must* dance. They were drawn in, as it were, by the contagion of example; and when once they had given way to it, they did not seem to know when to stop. This was kept up by music and by the encouragement and excitement of the crowd around; and it spread amongst classes of persons who (it might be supposed) would have had more power of self-restraint, and would not have joined such unseemly exhibitions. The extraordinary capacity, as it were, for enduring physical pain, was one of the most curious parts of this condition. They would frequently ask to be struck violently; would sometimes lie down and beg persons to come and thump and beat them with great force. They seemed to enjoy this.—In another case that I might mention this was shown still more. The case was of a similar type, but was connected more distinctly with the religious idea, and it occurred much more recently. The case was that known in medical history as the Convulsionnaires of St. Mêdard. There was a cemetery in Paris in which a great saint had been interred, and some young women visiting his tomb had been thrown into a convulsive attack which propagated itself extensively; and these convulsionnaires spreading the contagion, as it were, into different classes of French society, one being seized after another till the number became very great in all grades. Here, again, one of the most curious things was the delight they seemed to take in what would induce in other persons the most violent physical suffering. There was an organised band of attendants, who went about with clubs, and violently beat them. This was called the *grand secours*, which was administered to those who were subject to these convulsive attacks. You would suppose that these violent blows with the clubs would do great mischief to the bodies of these people; but they only seemed to allay their suffering.

This, then, is another instance of the mode in which this tendency to strange actions under the dominance of a particular idea will spread through a community. Here you have the direct operation of the perverted mind upon the body. But there are a

great many cases in which the perversion shows itself more in the mental state alone, leading to strange aberrations of Mind, and ultimately to very sad results in the condition of society where these things have spread, but not leading to anything like these convulsive paroxysms. I particularly allude now to the epidemic belief in Witchcraft, which, more or less, formerly prevailed constantly amongst the mass of the population, but every now and then broke out with great vehemence. This belief in witchcraft comes down to us from very ancient periods; and at the present time it is entertained by the lowest and most ignorant of the population in all parts of the world. We have abundant instances of it still, I am sorry to say, in our own community. We have poor ignorant servant girls allowing themselves to be—if I may use such a word—"humbugged" by some designing old woman, who persuades them that she can predict the husbands they are to have, or tell where some article that they have lost is to be found, and who extracts money from them merely as a means of obtaining a living in this irregular way, and I believe at the bottom rather enjoying the cheat. Every now and then we hear of some brutal young farmer who has pretty nearly beaten to death a poor old woman, whom he suspected of causing a murrain amongst his cattle. This is what we know to exist amongst the least cultivated of the savage nations at the present time, and always to have existed. But we hope that the progress of rationalism in our own community, will, in time, put an end to this, as it has in the middle and upper ranks of society during the last century or century and a half. It is not very long since almost everyone believed in the possession of these occult powers by men and women, but especially by old women. This belief has prevailed generally in countries which have been overridden by a gloomy fanaticism in religious matters. I speak simply as a matter of history. There is no question at all that this prevailed where the Romish Church was most intolerant, especially in countries where the Inquisition was dominant, and its powers were exerted in such a manner as to repress free thought and the free exercise of feeling; and, again, where strong Calvinism has exercised an influence of exactly the same kind—as in Scotland, a century and a half ago, and in New England, where there was the same kind of religious fanaticism. It is in these communities that belief in witchcraft has been most rife, has extended itself most generally, and has taken possession of the public mind most strongly; and the most terrible results have happened. Now I will

only cite one particular instance, that of New England, in the early part of the last century and the end of the century before. Not very long after the settlement of New England, there was a terrible outbreak of this belief in witchcraft. It began in a family, the children of which were out of health; and certain persons whom they disliked were accused of having bewitched them. Against these persons a great deal of evidence that we should now consider most absurd was brought forward, and they were actually executed: and some of them under torture, or under moral torture;—for it was not merely physical torture that was applied; in many cases it was the distress and moral torture of being so accused, the dread, even if found not guilty, of being considered outcasts all their lives, or of being a burden to their friends,—made confessions which any sober person would have considered perfectly ridiculous; but under the dominant idea of the reality of this witchcraft, no one interfered to point out how utterly repugnant to common sense these confessions were, as well as the testimony that was brought forward. And this spread to such a degree in New England, one person being accused after another, that at last, even those who considered themselves God's chosen people began to feel, "our turn may come next;" they then began to think better of it, and so put an end to these accusations, even some who were under sentence being allowed to go free; and to the great surprise of those who were entirely convinced of the truth of these accusations, this epidemic subsided, and witchcraft was not heard of for a long time afterwards; so that the belief has never prevailed in New England from that time to the present, excepting amongst the lowest and most ignorant class. In Scotland, these witch persecutions attained to a most fearful extent during the seventeenth century. They were introduced into England very much by James I., who came to England possessed by these ideas, and he communicated them to others, and there were a good many witch persecutions during his reign. After the execution of Charles I., and during the time of the Commonwealth and the Pu itans, there were a good many witch persecutions; but I think after that, very little more was heard of them. And yet the belief in witchcraft lingered for a considerable time longer. It is said that even Dr. Johnson was accustomed to remark, that he did not see that there was any proof of the non-existence of witches; that though their existence could not be proved, he was not at all satisfied that they did not exist. John Wesley was a most devout believer in witchcraft, and said on one occasion that if witchcraft was not to be believed, we

could not believe in the Bible. So you see that this belief had a very extraordinary hold over the public mind. It was only the most intelligent class, whose minds had been freed from prejudice by general culture, who were really free from it; and that cultivation happily permeated downwards, as it were; so that now I should hope there are very few amongst our intelligent working class in our great towns—where the general culture is much higher than it is in the agricultural districts—who retain anything more than the lingering superstition which is to be found even in the very highest circles—as, for instance, not liking to be married on a Friday, or not liking to sit down thirteen at the dinner table. These are things which even those who consider themselves the very aristocracy of intellect will sometimes confess to, laughing at it all the time, but saying, "It goes against the grain, and I would rather not do it." These, I believe, are only lingering superstitions that will probably pass away in another half century, and we shall hear nothing more of them; the fact being that the tendency to these delusions is being gradually grown out of.

Now this is the point I would especially dwell upon. To the child-mind nothing is too strange to be believed. The young child knows nothing about the Laws of Nature; it knows no difference between what is conformable to principles, and what, on the other hand, is so strange that an educated man cannot believe it. To the child every new thing that it sees is equally strange; there is none of that power of discrimination that we acquire in the course of our education—the education given to us, and the education that we give ourselves. We gradually, in rising to adult years, grow out of this incapacity to distinguish what is strange from what is normal or ordinary. We gradually come to feel—"Well, I can readily believe that, because it fits in with my general habit of thought; I do not see anything strange in this, although it is a little unusual." But, on the other hand, there are certain things we feel to be too strange and absurd to be believed; and that feeling we come to especially, when we have endeavoured to cultivate our Common Sense in the manner which I described to you in my last lecture. The higher our common sense—that is, the general resultant of the whole character and discipline of our minds—the more valuable is the direct judgment that we form by the use of it. And it is the growth of that common sense, which is the most remarkable feature in the progress of thought during the last century. The discoveries of science; the greater tendency to take rational and sober views of religion; the general habit of referring things to principles; and a number of influences

which I cannot stop particularly to describe, have so operated on the public mind, that every generation is raised, I believe, not merely by its own culture, but by the acquired result of the experience of past ages; for I believe that every generation is born, I will not say wiser, but with a greater tendency to wisdom. I feel perfectly satisfied of this, that the child of an educated stock has a much greater power of acquiring knowledge than the child of an uneducated stock; that the child that is the descendant of a race in which high moral ideas have been always kept before the mind, has a much greater tendency to act uprightly than the child that has grown up from a breed that has been living in the gutter for generations past. I do not say that these activities are born with us; but the tendency to them,—that is the aptitude of mind for the acquirement of knowledge, the facility of learning, the disposition to act upon right principles,—I believe is, to a very great degree, hereditary. Of course we have lamentable examples to the contrary, but I am speaking of the general average. I am old enough now to look back with some capacity of observation for 40 years; and I can see in the progress of society a most marked evidence of the higher general intelligence, the greater aptitude for looking at things as they are, and for not allowing strange absurd notions to take possession of the mind; while, again, I can trace, even within the last ten years, in a most remarkable manner, the prevalence of a desire to do right things for the right's sake, and not merely because they are politic. And I am quite sure that there is a gradual progress in this respect, which has a most important influence in checking aberrations of the class of which I have spoken.

Still we see these aberrations; and there is one just now which is exciting a good deal of attention,—that which you have heard of under the name of "Spiritualism." Now I look upon the root of this spiritualism to lie in that which is a very natural, and in some respects, a wholesome disposition of the kind—a desire to connect ourselves in thought with those whom we have loved and who are gone from us. Nothing is more admirable, more beautiful, in our nature than this longing for the continuance of intercourse with those whom we have loved on earth. It has been felt in all nations and at all times, and we all of us experience it in regard to those to whom we have been most especially attached. But this manifestation of it is one which those who experience this feeling in its greatest purity and its greatest intensity feel to be absurd and contrary to common sense—that the spirits of their departed friends should come and rap upon

tables and make chairs dance in the air, and indicate their presence in grotesque methods of this kind. The most curious part of it is that the spirits should obey the directions of the persons with whom they profess to be in communication,—that when they say "rap once if you mean yes, and rap twice if you mean no," and so on, they should just follow any orders they receive as to the mode in which they will telegraph replies to their questions. It seems to me repugnant to one's common sense; but the higher manifestations of these spiritual agencies seem to me far more repugnant to common sense; and that is when persons profess to be able to set all the laws of nature at defiance; when it is said, for instance, that a human being is lifted bodily up into the air and carried, it may be, two or three miles, and descends through the ceiling of a room. One of the recent statements of this kind, you know, is that a certain very stout and heavy lady was carried a distance of about two miles from her own house, and dropped plump down upon the table round which eleven persons were sitting; she came down through the ceiling, they could not state how, because they were sitting in the dark; and that darkness has a good deal to do with most of these manifestations. Now let us analyse them a little. I am speaking now of what I will call the genuine phenomena—those which happen to persons who really are honest in their belief. I exclude altogether, and put aside the cases, of which I have seen numbers, in which there is the most transparent trickery, and in which the only wonder is that any rational persons should allow themselves to be deceived by it.

I have paid a great deal of attention during the last twenty years to this subject, and I can assure you that I have, in many instances, known things most absurd in themselves, and most inconsistent with the facts of the case as seen by myself and other sober-minded witnesses, believed in by persons of very great ability, and, upon all ordinary subjects, of great discrimination. But I account for it by the previous possession of their minds by this dominant idea—the expectation they have been led to form, either by their own earnest desire for this kind of communication, or by the sort of contagious influence to which some minds are especially subject. I say "the earnest desire," for it is a very curious thing that many of those who are the most devout spiritualists are persons who have been themselves previously rather sceptical upon religious matters; and many have said to me that this communication is really the only basis of their belief in the unseen world. Such being the case, I cannot

wonder that they cling to it with very strong and earnest feeling. A lady, not undistinguished in the literary world, assured me several years ago that she had been converted by this spiritualism from a state of absolute unbelief in religion; and she assured me, also, that she regarded medical men and scientific men, who endeavoured to explain these phenomena upon rational principles, and to expose deception, where deception did occur, as the emissaries of Satan, who so feared that the spread of spiritualism would destroy his power upon earth, that he put it into the minds of medical and scientific men to do all that they could to prevent it. Now that, I assure you, is a fact. That was said to me by a lady of considerable literary ability, and I believe it represents, though rather extravagantly, a state of mind which is very prevalent; the great spread of the intense materialism of our age tending to weaken, and in some instances to destroy, that healthful longing which we all have, I believe, in our innermost nature, for a higher future existence, and which is to my mind one of the most important foundations of our belief in it. We live too much in the present; we think too much of the things of the world as regards our material comfort and enjoyment, instead of thinking of them as they bear upon our own higher nature. I believe that this tendency, which I think is especially noticeable in America—or at least it was a few years ago—from all that I was able to learn, had a great deal to do with the spread of this belief in what is called Spiritualism. The spiritualists assert that in America they are numbered by millions, that there are very few people of any kind of intellectual culture who have not either openly or secretly given in their adhesion to it. I believe that is a gross exaggeration; still there can be no doubt from the number of periodicals they maintain, and the advertisements in them of all kinds of strange things that are done—spirit drawings made, drawings of deceased friends, and spiritual instruction given of various kinds—that there must be a very extended belief in this notion of communication with the unseen world through these "media."

I can only assure you for myself that having, as I have said, devoted considerable attention to this subject, I have come to the conclusion most decidedly, with, I believe I may say, as little prepossession as most persons, and with every disposition to seek for truth simply—to allow for our knowledge, or I would rather say for our ignorance, a very large margin of many things that are beyond our philosophy—with every disposition to accept facts when I could once clearly satisfy myself that they were facts—I

have had to come to the conclusion that whenever I have been permitted to employ such tests as I should employ in any scientific investigation, there was either intentional deception on the part of interested persons, or else self-deception on the part of persons who were very sober-minded and rational upon all ordinary affairs of life. Of that self-deception I could give you many very curious illustrations, but the limits of our time will prevent my giving you more than one or two. On one occasion I was assured that on the evening before, a long dining table had risen up and stood a foot high in the air, in the house in which I was, and to which I was then admitted for the purpose of seeing some of these manifestations by persons about whose good faith there could be no doubt whatever. I was assured by them—"It was a great pity you were not here last night, for unfortunately our principal medium is so exhausted by the efforts she put forth last night that she cannot repeat it." But I was assured upon the word of three or four who were present, that this table had stood a foot high in the air, and remained suspended for some time, without any hands being near it, or at any rate with nothing supporting it; the hands might be over it. But I came to find from experiments performed in my presence, that they considered it evidence of the table rising into the air, that it pressed upward against their hands; —that they did not rest upon their sense of sight; for I was looking in this instance at the feet of the table, and I saw that the table upon which the hands of the performers were placed, and which was rocking about upon its spreading feet, really never rose into the air at all. It would tilt to one side or to the other side, but one foot was always resting on the ground. And when they declared to me that this table had risen in the air, I said, "I am very sorry to have to contradict you, but I was looking at the feet of the table all the time, and you were not; and I can assert most positively that one of the feet never left the ground. Will you allow me to ask what is *your* evidence that the table rose into the air?" "Because we felt it pressing upwards against our hands." I assure you that was the answer I received; their conclusion that the table rose in the air being grounded on this, that their hands being placed upon the table, they felt, or they believed, that the table was pressing upwards against their hands, though I saw all the time that one foot of the table had never left the ground. Now that is what we call a "subjective sensation;" one of those sensations which arise in our own minds under the influence of an idea. Take for instance the very common case—when we sleep in a strange bed, it may be in an inn that is not very clean, and we begin to be a

little suspicious of what other inhabitants there may be in that bed; and then we begin to feel a "creepy, crawly" sensation about us, which that idea will at once suggest. Now those are subjective sensations; those sensations are produced by the mental idea. And so in this case I am perfectly satisfied that a very large number of these spiritual phenomena are simply subjective sensations; that is, that they are the result of expectation on the part of the individual. The sensations are real to them. You know that when a man has suffered amputation of his leg, he will tell you at first that he feels his toes, that he feels his limb; and, perhaps to the end of his life, every now and then he will have this feeling of the limb moving, or of a pain in it; and yet we know perfectly well that that is simply the result of certain changes in the nerve, to which, of course, there is nothing answering in the limb that was removed. These subjective sensations, then, will be felt by the individuals as realities, and will be presented to others as realities, when, really, they are simply the creation of their own minds, that creation arising out of the expectation which they have themselves formed. These parties believed that the table would rise; and when they felt the pressure against their hands, they fully believed that the table was rising.

Take the case of Table-turning, which occurred earlier. I dare say many of you remember that epidemic which preceded the spiritualism; in fact, the spiritualism, in some degree, arose out of table-turning. My friend, the chairman (Dr. Noble), and I hunted in couples, a good many years ago, with a third friend, the late Sir John Forbes, and we went a great deal into these inquiries; and I very well remember sitting at a table with him, I suppose 25 years ago, waiting in solemn expectation for the turning of the table; and the table went round. This was simply the result of one of the party, who was not influenced by the philosophical scepticism that we had on the subject, having a strong belief that the phenomenon would occur; and when he had sat for some time with his hands pressed down upon the table, an involuntary muscular motion, of the kind I mentioned in my last lecture, took place, which sent the table turning. There was nothing to the Physiologist at all difficult in the understanding of this. Professor Faraday was called upon to explain the table-turning, which many persons set down to electricity; but he was perfectly satisfied that this was a most untrue account of it, and that the explanation was (as, in fact, I had previously myself stated in a lecture at the Royal Institution) that the move

ments took place in obedience to ideas. Movements of this class are what I call "ideo-motor," or reflex actions of the brain; and the occurrence of these movements in obedience to the idea entertained is the explanation of all the phenomena of table-turning. Professor Faraday constructed a very simple testing apparatus, merely two boards, one over the other, and confined by elastic bands, but the upper board rolling readily upon a couple of pencils or small rollers; and resting on the lower board was an index, so arranged that a very small motion of this upper board would manifest itself in the movement of the index through a large arc. He went about this investigation in a thoroughly scientific spirit. He first tied together the boards so that they could not move one upon the other, the object being to test whether the mere interposition of the instrument would prevent the action. He had three or four of these indicators prepared, and he put them down on the table so fixed that they would not move. He then put the hands of the table turners on these; and it was found, as he fully expected, that the interposition of this indicator under their hands did not at all prevent the movement of the table. The hands were resting on the indicator; and when their involuntary pressure was exerted, the friction of the hands upon the indicators, and of the indicators upon the table, carried round the table just as it had done before. Now if there had been anything in the construction of the instrument to prevent it, that would not have happened. Then he loosened the upper board and put the index on, so that the smallest motion of the hands upon the board would manifest itself, before it would act on the table, in the movement of the index; and it was found that when the parties looked at the index and watched its indications, they were pulled up as it were, at the very first involuntary action of their hands, by the knowledge that they were exerting this power, and the table then never went round. One of the strangest parts of this popular delusion was, that even after this complete exposure of it by Faraday, there were a great many persons, including many who were eminently sensible and rational in all the ordinary affairs of life, who said—"O, but this has nothing at all to do with it. It is all very well for Professor Faraday to talk in this manner, but it has nothing at all to do with it. We *know* that we are not exerting any pressure. His explanation does not at all apply to *our* case." But then Professor Faraday's table-turners were equally satisfied that *they* did not move the table, until the infallible index proved that they did. And if any one of these persons who *know* that they did not move the table, were to sit down in the same manner with those indicators, it

would have been at once shown that they did move the table. Nothing was more curious than the possession of the minds of sensible men and women by this idea that the tables went round by an action quite independent of their own hands; and not only that, but that really, like the people in the dancing mania, they *must* follow the table. I have seen sober and sensible people running round with a table, and with their hands placed on it, and asserting that they could not help themselves—that they were obliged to go with the table. Now this is just simply the same kind of possession by a dominant idea, that possessed the dancing maniacs of the middle ages.

Then the Table-talking came up. It was found that the table would tilt in obedience to the directions of some spirit, who was in the first instance (I speak now of about 20 years ago) always believed to be an evil spirit. The table talking first developed itself in Bath, under the guidance of some clergymen there, who were quite satisfied that the tiltings of the table were due to the presence of evil spirits. And one of these clergymen went further, and said that it was Satan himself. But it was very curious that the answers obtained by the rappings and tiltings of the tables always followed the notions of the persons who put the questions. These clergymen always got these answers as from evil spirits, or satisfied themselves that they were evil spirits by the answers they got. But, on the other hand, other persons got answers of a very different kind; an innocent girl for instance, asked the table if it loved her, and the table jumped up and kissed her. A gentleman who put a question to one of these tables got an extremely curious answer, which affords a very remarkable illustration of the principle I was developing to you in the last lecture—the unconscious action of the brain. He had been studying the life of Edward Young the poet, or at least had been thinking of writing it; and the spirit of Edward Young announced himself one evening, as he was sitting with his sister-in-law,—the young lady who asked the table if it loved her. Edward Young announced himself by the raps, spelling out the words in accordance with the directions that the table received. He asked, "Are you Young the poet?" "Yes." "The author of the 'Night Thoughts?'" "Yes. "If you are, repeat a line of his poetry." And the table spelt out, according to the system of telegraphy which had been agreed upon, this line:—

"Man is not formed to question but adore."

He said, "Is this in the 'Night Thoughts?'" "No." "Where

is it?" "J O B." He could not tell what this meant. He went home, bought a copy of Young's works, and found that in the volume containing Young's poems there was a poetical commentary on Job which ended with that line. He was extremely puzzled at this; but two or three weeks afterwards he found he had a copy of Young's works in his own library, and was satisfied from marks in it that he had read that poem before. I have no doubt whatever that that line had remained in his mind, that is in the lower stratum of it; that it had been entirely forgotten by him, as even the possession of Young's poems had been forgotten; but that it had been treasured up as it were in some dark corner of his memory, and had come up in this manner, expressing itself in the action of the table, just as it might have come up in a dream.

These are curious illustrations, then, of the mode in which the minds of individuals act when there is no cheating at all,—this action of what we call the subjective state of the individual dominating these movements; and I believe that that is really the clue to the interpretation of the genuine phenomena. On the other hand, there are a great many which we are assured of—for instance, this descent of a lady through the ceiling,—which are self-delusions, pure mental delusions, resulting from the preconceived idea and the state of expectant attention in which these individuals are. Here are a dozen persons sitting round a table in the dark, with the anticipation of some extraordinary event happening. In another dark seance one young lady thought she would like to have a live lobster brought in, and presently she began to feel some uncomfortable sensations, which she attributed to the presence of this live lobster; and the fact is recorded that two live lobsters were brought in; that is, they appeared in this dark séance—making their presence known, I suppose, by crawling over the persons of the sitters. But that is all we know about it—that they felt something—they say they were two live lobsters, but what evidence is there of that?—the séance was a dark one. We are merely told that the young lady thought of a live lobster; she said they had received so many flowers and fruits that she was tired of them, and she thought of two live lobsters; and forthwith it was declared that the live lobsters were present. I certainly should be much more satisfied with the narration, if we were told that they had made a supper off these lobsters after the séance was ended.

Now it has been my business lately to go rather carefully into the analysis of several of these cases, and to inquire

into the mental condition of some of the individuals who have reported the most remarkable occurrences. I cannot—it would not be fair—say all I could say with regard to that mental condition; but I can only say this, that it all fits in perfectly well with the result of my previous studies upon the subject, viz., that there is nothing too strange to be believed by those who have once surrendered their judgment to the extent of accepting as credible things which common sense tells us are entirely incredible. One gentleman says he glories in not having that scientific incredulity which should lead him to reject anything incredible merely because it seems incredible. I can only say this, that we might as well go back to the state of childhood at once, the state in which we are utterly incapable of distinguishing the strange from the true. That is a low and imperfect condition of mental development; and all that we call education tends to produce the habit of mind tha shall enable us to distinguish the true from the false—actual facts from the creations of our imagination. I do not say that we ought to reject everything that to us, in the first instance, may seem strange. I could tell you of a number of such things in science within your own experience. How many things there are in the present day that we are perfectly familiar with—the electric telegraph, for instance—which fifty years ago would have been considered perfectly monstrous and incredible. But there we have the rationale. Any person who chooses to study the facts may at once obtain the definite scientific rationale; and these things can all be openly produced and experimented upon, expounded and explained. There is not a single thing we are asked to believe of this kind, that cannot be publicly exhibited. For instance, in this town, last week, I saw a stream of molten iron coming out from a foundry; I did not see on this occasion,—but the thing has been done over and over again,—that a man has gone and held his naked hand in such a stream of molten iron, and has done it without the least injury; all that is required being to have his hand moist, and if his hand is dry he has merely to dip it in water, and he may hold his hand for a certain time in that stream of molten iron without receiving any injury whatever. This was exhibited publicly at a meeting of the British Association at Ipswich many years ago, at the foundry of Messrs. Ransome, the well known agricultural implement makers. It is one of the miracles of science, so to speak; they are perfectly credible to scientific men, because they know the principle upon which it happens, and that principle is familiar to you all—that if you throw a drop of water upon hot

iron, the water retains its spherical form, and does not spread upon it and wet it. Vapour is brought to that condition by intense heat, that it forms a sort of film, or atmosphere, between the hand and the hot iron, and for a time that atmosphere is not too hot to be perfectly bearable. There are a number of these miracles of science, then, which we believe, however incredible at first sight they may appear, because they can all be brought to the test of experience, and can be at any time reproduced under the necessary conditions. Houdin, the conjurer, in his very interesting autobiography—a little book I would really recommend to any of you who are interested in the study of the workings of the mind, and it may be had for 2s.—Houdin tells you that he himself tried this experiment, after a good deal of persuasion; and he says that the sensation of immersing his hand in this molten metal was like handling liquid velvet. These things, I say, can be exhibited openly—above board; but these Spiritual phenomena will only come just when certain favourable conditions are present—conditions of this kind, that there is to be no scrutiny—no careful examination by sceptics; that there is to be every disposition to believe, and no manifestation of any incredulity, but the most ready reception of what we are told. I was asked some years ago to go into an investigation of the Davenport Brothers; but then I was told that the whole thing was to be done in the dark, and that I was to join hands and form part of a circle; and I responded to the invitation by saying that in all scientific inquiries I considered the hands and the eyes essential instruments of investigation, and that I could not enter into any inquiry, and give whatever name I possess in science to the result of it, in which I was not allowed freely to use my hands and my eyes. And wherever I have gone to any of these Spiritual manifestations, and have been bound over not to interfere, I have seen things which, I feel perfectly certain, I could have explained, if I had only been allowed to look under the table, for instance, or to place my leg in contact with the leg of the medium. And it has been publicly stated within the last month, that the very medium whom I suspected strongly of cheating on an occasion of this kind, was detected in the very acts which I suspected, but which I was not allowed to examine. I cannot then go further into this inquiry at the present time; but I can only ask you to receive my assurance as that of a scientific man, who has for a long course of years been accustomed to investigate the curious class of actions to which I have alluded, and which disguise themselves under different names. A great number of the very things now done by persons professing

to call themselves Spiritualists, were done 30 years ago, or professed to be done, by those who call themselves "Mesmerists;" thus the lifting of the whole body in the air was a thing that was asserted as possible by mesmerists, as is now done by Mr. Home and his followers. These things I say, crop up now and then, sometimes in one form, sometimes in another; and it is the same general tendency to credulity, to the abnegation of one's Common Sense, that marks itself in every one of these epidemics.

Thus, then, we come back to the principle from which we started—that the great object of all education should be to give to the mind that rational direction which shall enable it to form an intelligent and definite judgment upon subjects of this kind, without having to go into any question of formal reasoning upon them. Thus, for example, is it more probable that Mr. Home floated out of one window and in at another, or that Lord Lindsay should have allowed himself to be deceived as to a matter which he admits only occurred *by moonlight?* That is the question for common sense. I believe, as I stated just now, that the tendency to the higher culture of the present age will manifest itself in the improvement of the next generation, as well as of our own; and it is in that hope that I have been encouraged on this and other occasions to do what I could for the promotion of that desire for self-culture, of which I see so many hopeful manifestations at the present day. When once a good basis is laid by primary education, I do not see what limit there need be to—I will not say the *learning* of future generations—but to their *wisdom*, for wisdom and learning are two very different things. I have known some people of the greatest learning, who had the least amount of wisdom of any persons who have come in my way. Learning, and the use that is made of it, are two very different things. It is the effort to acquire a distinct and definite knowledge of any subject that is worth learning, which has its ultimate effect, as I have said, upon the race, as well as upon the individual.

But there are great differences, as to their effects upon the mind, among different subjects of study; and I have long been of opinion that those studies afford the best discipline, in which the mind is brought into contact with outward realities,— a view which has lately been put forth with new force by my friend Canon Kingsley. You know that Canon Kingsley has acquired great reputation as an historian. He held the Professorship of History at the University of Cambridge for many years, and, in fact, has only recently withdrawn from it. Canon Kingsley also early acquired a considerable amount of scientific culture, and he has always been particularly

fond of Natural History. Now he lately said to the working men of Bristol that he strongly recommended them to cultivate Science, rather than study History; having himself almost withdrawn from the study of history, for this reason, that he found it more and more difficult to satisfy himself about the truth of any past event; whilst, on the other hand, in the study of science, he felt that we were always approaching nearer to the truth. A few days ago I was looking through a magazine article on the old and disputed question of Mary Queen of Scots, which crops up every now and then. She is once more put upon her trial. Was Mary Queen of Scots a vicious or a virtuous woman? The question will be variously answered by her enemies and by her advocates; and I believe it will crop up to the day of doom, without ever being settled. Now, on the other hand, as we study scientific truth, we gain a certain point, and may feel satisfied we are right up to that point, though there may be something beyond; while the elevation we have gained enables us to look higher still. It is like ascending a mountain; the nearer we get to the top, the clearer and more extensive is the view. I think this is a far better discipline to the mind than that of digging down into the dark depths of the past, in the search for that which we cannot hope ever thoroughly to bring to light. It so happened that only a fortnight ago I had the opportunity of asking another of our great historians, Mr. Froude, what he thought of Canon Kingsley's remark. He said, "I entirely agree with it;" and in some further conversation I had with him on the subject, I was very much struck with finding how thoroughly his own mind had been led, by the very important and profound researches he has made into our history, to the same conclusion—the difficulty of arriving at absolute truth upon any Historical subject. Now we do hope and believe that there is absolute truth in Science, which, if not at present in our possession, is within our reach; and that the nearer we are able to approach to it, the clearer will be our habitual perception of the difference between the real and the unreal, the firmer will be our grasp of all the questions that rise in the ordinary course of our lives, and the sounder will be the judgment we form as to great political events and great social changes. Especially will this gain be apparent in our power of resisting the contagious influence of "Mental Epidemics."

THE PROGRESS OF SANITARY SCIENCE.

A LECTURE,

BY PROFESSOR ROSCOE, F.R.S.,

Delivered in the Town Hall, Salford, December 19th, 1871.

UNDER THE AUSPICES OF THE MANCHESTER AND SALFORD SANITARY ASSOCIATION.

THE recent illness of the Prince of Wales may be said for several reasons to have been a good thing for the country; and, especially, because it has called attention, and that in a most marked manner, to sanitary matters. We cannot take up a newspaper now but we see it filled with letters on sewers and sewer gases. One suggests that every bad smell may bring to us typhoid fever, or some other disorder; whilst in another we read that these fears are mere illusions, and that in towns where there is a great deal of dirt, and where the ordinary rules of health are universally disobeyed, none of those dreadful ills occur which are painted so gloomily. Now, it is important that we should get to know as much as we can respecting the truth of these two assertions, so that on the one hand we may not be frightened with the idea that whenever we smell a bad odour we are sure to take typhoid fever; nor yet, on the other hand, be lulled into a false repose with regard to these matters, and think that sanitary laws can be broken with impunity. Equally false are both these points of view; and it is with the intention of pointing out some few of the distinct facts which science has been able to accumulate respecting the laws of health that I now address you.

In the first place, of the importance of the science of health there can be no doubt. Everybody wishes to be healthy, and

everybody, when he thinks of it at any rate, wishes to avoid such things as might bring him disease and suffering. How to preserve the health is not, however, so clear. For the most part men live in ignorance of those laws of health by which their action should be guided; and if we are asked how we should act under certain conditions, or whether such and such a state of things is an unhealthy one, many of us are unable to answer the question. One reason of this is the complicated and changing nature of the requirements. For instance, a man who lives under one set of physical circumstances will have to obey one set of laws of health; whilst men living under different circumstances will have to observe quite other laws in order to be healthy. The red Indian, roaming over the prairies, has to look out for altogether different dangers from those which surround us who live in crowded cities, where, perhaps, one thousand persons in some districts live on an acre. That the science of health is really less developed and less known than many other sciences lies, then, in the fact that it is more complicated than these other sciences, and a little reflection will show you why this is so. Thus, we find that enormous effects are produced by very minute causes; and this is the case not only when we catch a fever or a particular disease, without really being able to tell how we have caught it, or being able to assign to it any origin whatever; but we also find that this often holds good when we know that we are introducing a disease, as, for example, by the vaccine lymph, which, when introduced into the blood, though it be but the smallest particle on the point of a needle, produces a very extraordinary and valuable change on the human body. This, I say, shows us that the effect which is produced is enormously larger than the cause—larger not only than the apparent cause, but larger than the real cause. Hence, then, one great difficulty of determining these questions; and hence it is that men have lived for so many generations, and for so many hundreds and thousands of years, without having obtained even an imperfect knowledge of these subjects; for it is evident that we are only just at the threshold of knowledge as regards these matters; we are merely groping in the dark, and gradually getting hold of facts here and facts there and putting them together, in order to lay the foundation of this science of health of which we all stand so much in need.

If we look back we find that in the olden time, we see that whenever disease and epidemics broke out and spread over the country without apparent cause, the people attributed these afflictions to the visitation of God, or in heathen countries to the work of some

offended deity; and even now, in our times and in civilised countries, we find people who ought to know better wearing charms against certain evils, fancying that they will keep away disease. The first idea, then, we must get rid of in our investigation as to matters of health is this notion that disease is brought about by something indefinite and intangible, something which we must call upon the spirits of darkness or the spirits of light to deliver us from. We must first admit that there is a tangible cause for disease, a cause which we shall probably be able to find if we seek for it properly; but, at any rate, whether we find it or not, that a cause exists. It would be useless to attempt investigation unless we believed that there is a cause for every disease, and for every changing condition of the body which may occur. Very well, then, the first question is: can we arrive at such cause; can we put our fingers upon any cause or causes which do affect the general health of the community?

There is no doubt that if we look back at the history of disease, of epidemic disease especially, we shall find that the older epidemics, such as the plague, the sweating-sickness, and a number of these diseases, have, with the progress of time, gradually disappeared. We no longer hear of the plague in our cities. You have all read of the great plague of London in 1665, which was followed by the great fire of London; and it is said that London never would have been purified had it not been almost burnt down to the ground after this visitation. But now a-days we do not hear of these outbreaks of plague, at least in this country, and this is, doubtless, mainly to be attributed to general improvement in the style of living, and to care and cleanliness in getting rid of the impurities which the body throws off. I mention this to show that these epidemic diseases are in some way or other connected with causes which are removable, or, at any rate, which may be mitigated. Now, another fact that we have learned with regard to these epidemics of olden time is that they were most felt, and the mortality was always the greatest, amongst the poor, the dirty and the degraded portion of the population; as a rule these people suffered more than did those whose circumstances enabled them to live in a better way. The general conclusion is therefore that these epidemics are in some way assisted and abetted by dirt and degradation, and that improvement in the condition and habits of life of the people does either avert or lessen the virulence of these outbreaks of epidemic disease. This is shown by a vast number of facts; and the first that occurs to me is the case of the city of Buenos Ayres. You are aware that the year before last a most severe outbreak of yellow fever occurred in the large city of Buenos

Ayres, in the Brazils; and on investigation it was found that the sanitary arrangements of that city were of the very lowest and crudest character; that they had no drains, but only enormous cesspools which were never emptied, and under their tropical sun became festering masses of pollution and impurity. So strong was the conviction that this outbreak was due to the unhealthy arrangements of their city, that the authorities resolved to spend an enormous sum, I believe something like four millions sterling, on a complete system of drainage and water supply for the city. They are going to remodel their whole arrangements, and do away with these festering nuisances, in the belief, which I have no doubt will be justified by the result, that they will thereby prevent such an outbreak in the future.

The question as to the mode in which an individual or a community becomes infected divides itself into two distinct branches of epidemic diseases. First we have to consider why the epidemic comes at certain intervals; why, for instance, the cholera never visited us before 1831, why it then disappeared and after a lapse of years again breaks out? Next we have to ask how is the disease propagated when it has once broken out. As regards the first question I think we have as yet very little safe ground from which to draw conclusions. That the march of the cholera in a westerly direction can generally be traced and its probable occurrence foretold is quite true, and that plausible theories have been proposed to account for the possibility of the existence of cholera in certain countries at certain times is also true. Still on the whole our knowledge on this question is of the most incomplete character. Not so with regard to the second part of our inquiry as to how this particular epidemic disease is propagated. In an inquiry as to the cause of production of any disease, we may take it for granted that the material causing the disease must be brought to the individual either in the water we drink, or in the air we breathe, or in the food we eat. I am not speaking now of what are termed "hereditary diseases," which are of a totally different character, and do not come into the class of those which can be removed by sanitary improvements. Applying this principle to the case of cholera, as being one of the best investigated of epidemics, we find that the poisonous matter which is the cause of this disease is very frequently, at any rate, taken with the water that is drank. In order to make this matter clear to you I will only call your attention to two or three cases of evidence as to the truth of the statement. The first is from that given before the Royal

Commission on the water supply of the metropolis, by Mr. Simon, the medical officer of the Privy Council. Mr. Simon says :—

"It is, I believe, a matter of absolute demonstration that in the old epidemics, when the south side of London suffered so dreadfully from cholera, the great cause of the immense mortality there was the badness of the water then distributed in those districts of London. In the interval between the 1849 epidemic and the 1854 epidemic one of the two companies which supply the south side of London had amended its source of supply; it had gone higher up the river, and we at once lost a great part of the mortality on that side of the river. But it was found that this great difference did not prevail uniformly through the south side of London, but was confined to those houses which were supplied from the amended source. There was still a great mortality on the south side of the river, but this belonged exclusively to the houses which were still supplied with impure water."

From a table given in the report from which I quote it is seen that the number of deaths per thousand from cholera in the visitation of 1848, in the houses supplied by the Lambeth Company, was 12·5; at the next visitation the same houses lost only 3·7; that is to say, that the rate had diminished by three-fourths; whilst in the houses supplied by the Vauxhall Company the death rate at the first visitation was 11·8, and in the second visitation 13; so that the death-rate had actually increased in the houses which were supplied with water from the company which had not mended its ways.

Another epidemic, that of 1866, only confirmed the conclusions drawn from previous experience, for Mr. Simon clearly shows that the heavy mortality in this year fell in the east of London, and was distinctly confined to a district supplied by water drawn from a foul part of the river Lea and containing sewage impurity.

A third instance is that singular case known as the Golden Square case. In the course of five or six days, from the 30th August, 1854, not less than about 500 persons died of cholera in a district in London, round Golden Square, containing about 5,000 inhabitants. Upon investigation it was found that nearly all the people who died had been drinking water from a pump in Broad Street, which was thought to yield very excellent water, but was afterwards found to communicate with a cesspool in an adjoining house. These cases clearly prove that contaminated water may produce cholera

We will next take the disease from which the Prince of Wales

has suffered, and which is known as typhoid or enteric fever. This disease is generally supposed to be caused either by drinking impure water, or by breathing the foul gases generated in sewers; and it is said that 20,000 persons die annually from this preventable disease. The preventable nature of this disease is so generally acknowledged, that when an outbreak of typhoid fever occurs in a district, the medical department of the Privy Council—a most important department, and one which will become of greater influence still, from the act of Parliament passed last session—sends down a duly qualified medical man to inquire into the causes of the origin and spread of such an epidemic outbreak. Dr. Buchanan was sent down in September, 1867, to investigate the cause of the outbreak of typhoid fever at Guildford. He reported that a new well had been sunk to supply the higher part of the town, and that water from this well was supplied to about 330 houses for one day only, the 17th August. On the 28th of August there were several cases of typhoid fever in these houses, although they are all situated in the highest and healthiest district in the town. The number daily increased, and there were in all about 500 cases and 21 deaths. With three exceptions, all the persons attacked in August and September had drank the water exceptionally supplied for one day only—as just stated. It was subsequently found that a sewer ran within ten feet of the well, and that the sewage leaked through the joints of the brickwork and saturated the soil just above the spring which supplied the well.

I might give you a great number of other instances of a similar character. I will content myself by stating that Dr. Parkes, the well known Professor of Sanitary Science in the medical school at Netley, has collected a good deal of evidence as to diseases which may be communicated by water, not only to the troops, but among the civil population; and he has made a list of diseases, all of which may be communicated by means of water, and amongst these he has collected many instances of local outbreaks of typhoid fever arising from water impregnated with typhoid sewage or possibly simple sewage. One case quoted by Dr. Parkes is that of a young ladies' school, where infiltration of sewage into the well supplying the house with water was shown to be the cause of a severe outbreak of typhoid fever.

These cases prove to us that epidemic diseases may be produced and have been produced by drinking impure water. Having assured ourselves of this, let us next see what chemistry can tell us respecting our means of detecting whether the water

used for drinking is pure or impure. You will understand that the danger lies in the water being impregnated with animal decomposing matter, and with sewage matters generally. Now, although, chemists, like other men, cannot do all that they would like to do in these investigations, still they can do something; and I wish to point out to you what chemistry can tell us respecting the purity or the impurity of such water. In the first place let us clearly understand that neither the chemist, nor the physician, nor the microscopist, nor the physiologist, can tell us whether the water contains typhoid poison, or whether the water contains cholera poison or whether the water contains the poison of any other particular disease. There are no means of ascertaining this, even with the most poisonous exhalations from the cholera patient, except it be the actual test of the action of the poison on a human subject. The microscopist cannot detect, for instance, in the rice water from a cholera patient, that there are any particular germs of cholera poison in that offensive liquid; and yet if the smallest quantity of it got into the digestive organs of a man it would produce cholera. But although the chemist is unable to do this, he is able to tell the difference between a pure water and a water which contains animal impurity; and if the water contains cholera poison, or the germs of typhoid, or of some other disease, or simply animal excrementitious matter, it is, I need scarcely tell you, unfit to drink; and the chemist can help us to detect such matters.

Now what is it that the chemist can do in this respect? You know that all animal matter makes a disagreeable smell when it is burnt. The difference between burning a feather and burning a piece of wood is evident to your senses. Now, this burnt feather smell is caused by the presence of a body which the chemists call Nitrogen, which exists in the air, but which also enters as a characteristic ingredient into all animal matter. In this respect animal bodies differ from the bodies of vegetables. Now, when the decomposition of an animal body occurs, the nitrogenous portions which are thrown off, that is the liquid and the solid products, get into the sewers; and if we can find in water a large quantity of this nitrogenous animal matter, we may be certain that that water is not fit to drink. I cannot explain to you to-night how the amount of nitrogenous matter contained in water is ascertained; but if you will look at these analyses taken from Professor Frankland's report on the Chemical Composition of the Lancashire rivers, you will see what I mean.

Composition of Lancashire Rivers.

Parts in 100,000.

	Irwell.		*Mersey.*	
	*1	2	3	4
Total solid soluble...............	7·8	55·80	7·62	39·50
Organic carbon	0·187	1·173	0·222	1·231
Organic nitrogen	0·025	0·332	0	0·601
Ammonia	0·004	0·740	0·002	0·622
Nitrogen as nitrates and nitrites	0·021	0·707	0·021	0
Total combined nitrogen	0·049	1·648	0·023	1·113
Chlorine	1·15	9·63	0·94	—
Hardness temporary	3·72	15·04	4·61	10·18
Total hardness	3·72	15·04	4·61	10·18
Suspended Matter.				
Organic	0	2·71	0	
Mineral	0	2·71	0	—
Total	0	5·42	0	-

*1. The Irwell near its source.
2. The Irwell below Manchester.
3. The Mersey, one of its sources.
4. The Mersey below Stockport.

We have here the composition of Lancashire rivers taken from the admirable report of the Rivers Pollution Commission. In the first column you have the analysis of the river Irwell, that is of the water taken at its source, where it is as pure as we could wish water to be, being, in fact, very much like the pure water which the Manchester corporation supply to us from the Derbyshire hills. In the second column you have the composition of the Irwell below Manchester. In the same way you will see the composition of the Mersey at its source, and its composition below Stockport. Let us confine ourselves to the Irwell. Now, in the first place, you will notice that the total soluble matter, or that which is dissolved in the water, is very much more, as you may imagine, when the Irwell gets below Manchester than it is at its source. But this total soluble matter might be perfectly innocuous; it might, for instance, be common salt, or carbonate of lime, or gypsum, or any other substance which might not be hurtful. But the next constituents which we find on this list are most hurtful; these are the organic carbon and the organic

nitrogen, and these are hurtful because they serve as a measure of the vegetable or animal matter which the water contains. Observe the difference in the two kinds of water. You see that in the Irwell below Manchester there is nearly ten times as much organic carbon as there is in the water when taken at its source; and that there is more than ten times as much organic nitrogen (derived solely from animal sources) below Manchester as there is at its source. The next two substances we have to notice are the ammonia and the nitrogen, as nitrates and nitrites, both of which, although harmless in themselves, are products of the oxidation of animal matter, and therefore signs of previous pollution. The quantities of ammonia and nitric acid in the pure Irwell water are almost nothing, whilst below Manchester they are increased, you see, 300 or 400 times. If we next look at the total combined nitrogen contained in the water, we find for 49 parts in the pure Irwell water we have 1,648 parts in the impure water below Manchester! Thus we see that by a chemical analysis of water, we can at once detect by the organic, or albumenous nitrogen, whether it still contains animal impurity, and by the ammonia and nitric acid whether the water has been polluted by animal matter which has since been destroyed, or, by the absence of excessive quantities of these nitrogenous bodies, whether the water has never been in contact with animal matter. It is thus possible to calculate by a very simple process how much sewage has come into such a water. Let us, for instance, take this one case. It is found that in 100,000 parts of average London sewage there are 10 parts of nitrogen existing, as ammonia and nitrates, derived from the oxidation of animal matter. Now, supposing 100,000 parts of Irwell water was found to contain 10 parts of nitrogen, we should say that the Irwell water is just as strong as London sewage, that is, equal to the average composition of the water taken out of London sewers. If it contained five parts in 100,000, we should say that it was just half as strong; or we might then say there are just equal parts of pure water and London sewage in the river Irwell. Now what is the amount we find in the Irwell? We find that the nitrogen, as ammonia and nitrates, as you see in that table, is 1·447 (0·740 + 0·707). Very well; now there is also a small quantity of nitrogen, as ammonia and nitric acid, contained in rain water, but the quantity is exceedingly small. If we therefore subtract the quantity which is found in rain (viz., 0·032 part in 100,000) from the quantity which is found in the Irwell (viz., 1·447), we shall have the quantity (1·415) which is due to the sewage impurity in the

Irwell, and we can then easily calculate how much London sewage this corresponds to. It evidently corresponds to 14,150 parts of London sewage. Thus you see that 100,000 parts of the Irwell water below Manchester contain the quantity of nitrogenous animal impurity which is contained in 14,150 parts of London sewage; in other words—so far as regards the animal impurity—if you were to take 86 gallons of pure water and mix with them 14 gallons of London sewage, you would have the composition—so far as animal impurity goes—of 100 gallons of Irwell water. What I want to prove is that we have in this way a measure of the impurity of water, so that when we have made our analysis we can calculate how much previous sewage contamination the water has undergone.

In diagram No. 2 you see the composition of the Manchester Corporation water:—

MANCHESTER CORPORATION WATER, 1868,

Contains in 100,000 *parts*—

Total solid impurity	6·20
Organic carbon	0·183
Organic nitrogen	0·009
Ammonia	0·006
Nitrogen, as nitrates and nitrites	0·025
Total combined nitrogen	0·039
Previous sewage contamination	0·000
Chlorine	1·120
Temporary hardness	0·14
Permanent hardness	3·59
Total hardness	3·73

You see that there is no previous sewage contamination; but in all river water we find from the drainage of houses or towns previous sewage contamination; and it is therefore possible for us to make the prediction that in the visitation of cholera which this country is almost sure to undergo next summer, Manchester will pass nearly unscathed, while London, being still supplied by river water, will suffer from the epidemic. The point I want you to understand is that the chemist—thanks chiefly to the labours of Professor Frankland—is now able to estimate this previous sewage contamination.

Now, although I cannot show you how the amount of the nitrogen is ascertained, I can show you in another way the dif-

ference between Irwell water and our drinking water. In this glass jar we have some pure water, as supplied to us by the Corporation of Manchester. Here we have another clear-looking water, not quite so nice and clear as the drinking water, but still a very respectable water, which you might wish to drink and fancy that it would not be so bad, though the taste might not be so nice as the pure water. This is filtered water taken from the black stream which flows past our doors—the river Irwell. I have here a red liquid which will oxidise animal impurities and destroy them, and thereby lose its own colour. You will find that one drop of this coloured solution—permanganate of potash—will be sufficient to colour this pure water, because there is no impurity in it which requires oxidation. I will put in three drops, which will render the water pink. Now I will take the Irwell water and add many drops of the permanganate. Let us see what happens here. This Irwell water, you see, soon becomes colourless, showing that it contains organic matter capable of undergoing oxidation, and therefore in a condition of decomposition or putrefaction, and you see I have to add a considerable quantity yet until I get a permanent pink colour. And, therefore, although this method of testing water is not so accurate a one, or to be relied on so implicitly as the determination of the nitrogenous impurity, yet it is one which is of value, and which I have no difficulty in making visible to you, thus demonstrating to the sight that the clear Irwell water is impure.

There is still another means which chemists have of telling whether water is pure, and that is by the presence of common salt. Pure spring water ought to contain very little common salt; but water which contains the infiltrations of sewage brings in with it a large quantity of common salt derived from the urine. Any water which contains more than one part of common salt in 100,000 is almost sure to have that salt brought in by sewage, and will therefore be impure. This does not apply, of course, to water flowing through salt districts. The springs and rivers of Cheshire in some places contain large quantities of salt which does not come from sewage; but I am speaking of places in which there is no occurrence of rock salt. Thus you see that we have three means of detecting and determining the amount of organic impurity in water—first, the nitrogen; second, this test with the red permanganate; and, thirdly, the presence of common salt; and it is clear that the chemist is able to detect organic impurity in water, and to tell positively that such and such a water is a pure one, and that such and such a water is an impure

one and unfit and dangerous or even fatal to drink; so that although he is not able to say that a certain water contains cholera poison, he is able to say that the water is poisonous.

Next about the air we breathe. You know that the air contains oxygen, nitrogen, and carbonic acid. Oxygen is the vital air. I can show you very easily that air consists of two different things. I take this glass cylinder, which is filled with air. This cylinder contains five volumes of air. I will burn a bit of phosphorus in it, and you will very soon see that the phosphorus will go out. After a little while these white fumes will disappear, and we shall see that we have not got as much air as we had before-- about four volumes will be left; we shall also see that the gas which is left, called nitrogen, has different properties from common air, inasmuch as a light will go out in the gas which is left. The oxygen gas, which we use in breathing, is a colourless invisible gas, in which bodies burn with far greater brilliancy than they do in the air. If we take a little bit of charcoal, for instance, and burn it in this oxygen, you will see that it will burn much more brilliantly than it does in ordinary air. Now besides these two gases—oxygen and nitrogen—we have a third gas in the air, called carbonic acid gas. This gas is given off whenever bodies such as charcoal, coal, or candles burn in the air; it is also given off by our breathing, as you know. This will be made evident if I blow into this lime water, which will become turbid from the presence of this carbonic acid coming from the lungs. Well, then, we have in the air the oxygen, or the vital air; the nitrogen, or the non-vital air; and the carbonic acid, which we may call the choke damp. The carbonic acid plays a very important part as regards plants, because it serves as their food; but it renders the air impure for the use of animals, and it is produced by the combustion of bodies. That this is the case I can show you by a very simple experiment. We have here a lamp burning under a jar, and the products of the combustion come out through this chimney. If I hold a clean plate of glass above this aperture, you will see that a large quantity of vapour of water comes out, the result of this burning of the gas. There you see the glass is bedewed with moisture. Now let us stop the door of our glass house with a piece of putty, and observe what takes place. The flame, you see, becomes longer and more smoky, and in a very short time it will go out, because there is not a sufficient supply of oxygen to keep up the combustion; and if we hold this glass plate over it now the plate does not become bedewed with moisture, because there is no draught

through the pipe, and no mode by which the vitiated air can escape. This illustrates to you the principle of ventilation. Wherever a candle can burn, there an animal can live; but where the candle goes out, there as a rule the animal also goes out and cannot live. Here you see the gas flame is very nearly gone out. I will now open the door again and let some fresh air in, and I think in a short time that the flame will revive, and the combustion go on much as before. Now the air that we give off from our lungs is impure, because it contains carbonic air; a candle cannot burn in it. You have all heard the story of the Black Hole of Calcutta, and you know that when men are shut up in a close room in which they cannot get any supply of fresh vital air or oxygen, they cannot live, they are suffocated. I have shown you that if we vitiate the air in this bell jar by contaminating it with carbonic acid gas, through the withdrawal of the oxygen from it, the candle will not burn. The candle burns in this jar which contains air, but if we now breathe this air once or twice, you will observe the effect upon the combustion of the candle. There, it has been breathed once; now we will breathe it once again. The candle now burns very dimly. With one further breathing of the air we shall so diminish the quantity of oxygen, and increase that of the carbonic acid, that the candle will go out. Here, then, you see at once the necessity for the ventilation of your rooms. All this has been long well known, and I only introduce these facts because they help to give you a general notion of what chemistry tells us about the composition of the air.

There is, however, still another constituent of the air of still greater importance, as regards our health, even than this carbonic acid, about which our knowledge is newer and less perfect, and that is Organic Matter. You all know what we mean by a "close room;" you all know that if you do not sleep with your windows open, as you ought to do—if you sleep with your windows shut, and especially if you have no fire-place in your room, when you come back to the room from the fresh air, before opening the window, you notice a disagreeable close smell. That smell ought never to exist in the room; for it shows that you have something there which is neither oxygen, nor nitrogen, nor carbonic acid, inasmuch as all these gases have no smell; but it is organic matter—emanations from the bodies of those who have slept in that room. These organic emanations or substances existing in the air are most dangerous, and do much towards spreading epidemic diseases, as far as

the air is concerned. What does science teach us with regard to this organic matter in the air? This, again, like the organic matter in water, is not an easy matter to investigate, and in many cases we are as yet quite in the dark concerning its mode of action or constitution. Still it is not difficult to show that organic matter is contained in the air, and that some of these organic substances are gases and some of them solid bodies. Thus if we look at the air of our rooms when the sun is shining in upon it, what do we see? We see what we call "motes" dancing in the sunbeam. What are those motes? They are finely divided bits of all sorts of things—bits of skin, of the epidermis; bits of clothing; dust from the street; bits of stones and bits of iron—a thousand different things, and all so small that they do not settle down in the air—at any rate not for a long time—but continually dance up and down as we see them in the sunbeam, and are as continually being breathed in to our lungs. We do not see these motes when the sun is not shining, not because they are not there, but because they are too small to be seen except when the sunlight strikes upon them and reflects the light back into our eye. That a number of these little things are germs, seeds, or spores of various kinds, has been proved by a great number of experiments. If we wish to prove the organic nature of these particles, we may collect this fine aerial dust by drawing air through something upon which the dust can be filtered out, as upon a piece of cotton wool; and if we then put this cotton wool with the dust upon it into a solution of sugar, we find that that dusty cotton wool can produce all sorts of changes in the sugar—changes which do not occur if we keep out this dust, as we can do—and thus we can show the production from the dust not only of living vegetables but also of living animals. This experiment has been made by our townsman, Dr. Angus Smith, than whom nobody has done more to advance our knowledge concerning the organic matter in the air. Dr. Angus Smith, as long ago as 1848, made the following experiment: he placed a little pure water in a glass bottle and took it into a room where a number of people were present, and very often shook this water up with the air in the bottle, pumping in a fresh supply of air and shaking it up again many hundred times. He then, with his friend Mr. Dancer, examined the nature of the water which was in the bottle, and they found that this water, after a little time, contained living animal organisms—little vibrios, as they are termed—very minute, but still distinct animal forms, which are well known to those who occupy themselves,

as Mr. Dancer has done, with the study of the very smallest and lowest creatures, both animal and vegetable, which can only be seen under the microscope. So that of the existence floating in the air of these germs or eggs—if you like to call them so—of the animals there can be no doubt. Now, then, comes the other question how far these little germs which exist in the air, can produce disease? About this, satisfactory evidence is, of course, more difficult to obtain. It has not, so far as I know, been positively proved that these little germs are always the cause of disease, for in many cases the general dissemination of these germs has proved compatible with a healthy condition of the people; but that they may, and sometimes do, produce disease we have abundant evidence to prove. Now the question to which I wish again to direct your attention is, can the chemist determine whether the air is pure or whether it is impure as regards these organic matters? You will say, "we do not want the chemist to do this, because we can smell when the air is impure." But the answer to this is, you cannot always smell when air is impure any more than you can taste when water is impure; thus the fever and ague-producing air of the marshes is quite free from smell, and yet capable of giving rise to most serious diseases. You therefore require something more than your unaided senses, and the chemist can help in this matter; for although he cannot tell whether there are germs present which will produce certain diseases, he can tell whether there is or is not organic matter in the air, and whether it exists in such quantity as to make the air not fit to be breathed for any length of time. In this diagram you see the amount of organic matter contained in the air, according to the experiments of Dr. Angus Smith:—

Relative Amount of Organic and Oxidizable Matter in the Air.

(Angus Smith.)

St. Bernard's Hospice	2·8
Hill in Lancashire	2·8
Lake in Lucerne	1·4
At sea, 60 miles from land	3·5
Kew Gardens	10·0
Finchley	15·0
London, Waterloo steps	42·0
London, Southwark Bridge	55·0

Dr. Angus Smith found in pure air—obtained from St. Bernard's Hospice, on one of the passes over the Alps—a very small quantity (2·8 parts) of this organic matter; but in Manchester, in the air of his own laboratory, he found 48 parts; in the air over the Lake of Lucerne 1·4; in the air of a pigstye 70; he goes away to sea, and at 60 miles distance, finds $3\frac{1}{2}$ parts; in the Greenheys fields, with the wind blowing from Manchester, 40 parts In the neighbourhood of towns he finds less impurity than in towns themselves. Kew and Finchley air shows much less than that taken from near London, Waterloo or Southwark bridges, or from Lambeth. In Manchester, near one of the sweet streams I have referred to, with its strong smell of putrefaction, he got as much as 73 parts of organic matter. These numbers, you will understand, do not give absolute quantities, but they show the difference of pure and impure air as regards this organic matter.

We have heard a great deal lately about sewer gases, and there is no doubt that not only is a general lowering of the tone of the body produced by breathing air vitiated by the entry of sewer gases into houses, but that actual danger to life ensues from the bringing these impure gases, which may contain the germs of specific disease, into our dwelling-houses. But I think we ought to be careful, especially at the present moment, from letting the impression get abroad, that wherever there is a bad smell we are in danger of our lives. The public are very apt to run into extremes. At one time they don't think at all about the matter, but when attention is called to the subject by such an event as the illness of the Prince of Wales, they are apt to fancy that whenever they perceive a bad smell they are sure to be dreadfully ill. Still, as I have shown you, there is no doubt that organic germs exist in the air, and that air coming into houses from sewers, by bringing in these floating germs, must be a constant source of danger, and may become a source of fatal disease. But that effluvia and evil smells from decomposing animal matter are not invariably, or even generally, accompanied by epidemic outbreaks is a fact which common experience proves, though in localities where such effluvia exists the epidemic poison, when it comes, appears to find favourable ground for its growth, and the place at once becomes a hotbed of disease. This view is confirmed by the recent report issued by two very distinguished physicians, Drs. Burdon Saunderson and Parkes, on the sanitary condition of Liverpool. They distinctly say, considering the high death rate in the lowest parts of that town and finding that there has been no

outbreak of typhoid fever, that they see no reason to attribute that high death rate chiefly, if at all, to the escape of these sewer gases into the houses: so that as far as Liverpool is concerned, the blame of the high death rate does not seem to lie at the door of the sewer gases.

I should wish next to bring before you a very remarkable example of what exact scientific investigation can do to help us to a knowledge of these most complicated and difficult questions as to the causes of the propagation of epidemic disease. You know that France is one of the great silk-producing countries; and you know that the silk is spun by a small caterpillar or worm that lives on mulberry leaves, and that it is reared largely in the south of France. You are all, I dare say, also aware of the changes which this silkworm undergoes—that the worm changes its skin several times, and that, having attained a certain growth, a peculiar secretion, which forms the silk, is produced inside the animal, which then spins its cocoon and retires into the inside—forming what we know as the chrysalis. After some time this chrysalis appears as a moth, which lays its eggs and dies, and a fresh generation of worms make their appearance from the eggs. Now the value of the productions of the silk trade in France is something enormous. In 1853 the silk produced in France was worth 136 millions of francs. Unfortunately, soon after that year a fatal epidemic, called pébrine, broke out amongst the silk-worms. Everything was done and every nostrum and contrivance tried to stop this epidemic, but nothing succeeded, and the silk-worms continued to die. The peculiar symptoms of the disease were that black spots came out all over the caterpillars, and their silk secreting power was altogether lost. This went on until, in 1864, the value of the silk made in France amounted to only four millions of francs; so that the disease caused a loss of about 100 millions of francs per annum. The worms—both the healthy and stricken ones—had been carefully examined, and it was found that when they died of this disease they were almost filled with masses of little globular corpuscles, so that the place where the silk ought to have been contained nothing but these disease-bringing globules. Nobody, however, could tell how to stop the epidemic. It was found that sometimes, when the disease could not be detected either in the egg or in the caterpillar (which spun silk), the next generation of apparently healthy caterpillars which came from apparently healthy moths became diseased, and produced no silk. In short, the disease baffled all investigation. But some time after this dreadful state of things, the celebrated

French chemist, Pasteur, was asked to try what he could make of it. Now Pasteur had previously paid great attention to this particular subject of organic germinal matter in the air, and he succeeded in fathoming the whole difficulty. He proved what the disease was occasioned by, and showed how it might be prevented. I will give you an idea how Pasteur found this out. In the first place, I told you that the healthy caterpillar might produce unhealthy moths, or moths that laid bad eggs; but Pasteur found that this was because the particles of diseased matter existing in the caterpillar supposed to be healthy were so small that they could not be seen by the best microscopes. He investigated the matter step by step with scientific precision, and he found that by examining the moth instead of the caterpillar he could invariably tell whether the moth was a sound moth and would lay sound eggs, or whether it was an unsound moth and would lay unhealthy eggs, which afterwards would give birth to a stricken or diseased caterpillar. He proved this completely; and moreover he showed that not only could he tell by examining the moth that these little globules existed in the moth, although not apparent in the caterpillar, but that the caterpillar could become infected, although it did not receive the disease by transmission, by contact with another unhealthy caterpillar. And in this way, by most carefully guarding against a caterpillar becoming infected by a neighbouring one, and by most jealously taking care that all the moths which laid eggs, or whose eggs were kept, were healthy moths, he entirely got the disease under his control, and the result is that the disease is now almost passing away. I will not take up your time now by reading, as I intended, a passage from his paper, but I will simply say that in this way he was able to point out the cause of the disease, and thus to prevent the great pecuniary loss which France had been suffering. Here, then, you have a clear case in which careful scientific examination was successful in explaining a complicated and apparently insoluble difficulty; and there can be little doubt that the application of similar methods of exact investigation to the cases of other epidemic diseases will in the end show that every such disease is capable of being, if not altogether prevented, at any rate greatly lessened.

In conclusion I wish you to understand that, whatever progress men of science may make in the discovery of the cause of epidemic disease, and however completely our imperial or municipal authorities may carry out preventive and curative measures founded upon such discoveries, it rests in the end with the people to say whether such measures shall be productive of good or whether they shall

remain a dead letter without influence on the mass of the population. All the discoveries of science, all the care of our authorities can avail nothing, when the people themselves are dirty, dissolute, drunken, and degraded. This debased condition of the population is the most powerful cause of the high death rate of our towns, and this at present far outweighs the evil effects produced by drinking water contaminated with sewage, or by breathing air rendered impure by sewer gases.

THE RAINBOW.

A LECTURE

By PROFESSOR ROSCOE, F.R.S.,

Delivered in the Hulme Town Hall, Manchester, October 29th, 1872.

I SUPPOSE there are very few of us here present who have not in some idle moment thrown stones into a pool of water, and watched the beautiful rings which spread, and spread, and spread, from the place where the stone fell into the clear, still water. There are some of us, perhaps, too, who have seen this on a larger scale, who have been on the Atlantic or some other large ocean when the winds blew and the billows rose, and it seemed almost beyond hope that the ship would ever reach land again. Those who have seen both the little waves on the pool and the large waves on the ocean will have got an idea, I dare say, that these waves move forward with immense velocity—that the water is thrown in a mass forward. This idea is, however, not altogether true; and I wish to try to show you of what this wave-motion in the little pool, as well as the gigantic billows of the Atlantic, consists. If you make a very simple experiment for yourselves, you will find that the water does not rush from the place where the stone is thrown in to the outside of the pool with the waves that go to form these beautiful circles; for if you pour a little ink into the centre of the pool, or where you throw your stone in, you will find that the ink or coloured water is not carried forward with the wave. Therefore, you see that it is not that the particles of water where the stone is thrown in pass to the outside, but

rather that this wave-motion is caused by a propagation of an up-and-down or vibratory motion of the particles of water. This wave-motion is well shown to us if we take a string or cord, having round balls fixed upon it. If I now stretch this cord, and shake it, I produce what I have no doubt you will all see, namely, this peculiar wave in the cord. Here we have the wave passing along the cord as it shakes. The wave may be large or it may be small, but it is always produced by the varying up-and-down motion of the black balls fixed along the cord at regular intervals. Imagine each of these little black balls to be a particle of water, and you get a notion of the way in which the particles of water move in the wave. This wave-motion, caused by the vibration of single particles, can be more strikingly shown to you by this arrangement, in which I have a number of ivory balls attached to iron rods, which can all be made to move up and down in different positions. Suppose these little white balls to illustrate the particles of water. Each of these particles will perform a slight oscillation, but the whole gives the effect of a wave-motion; and you now see the wave travelling forwards. This shows you, then, that the particles of water do not themselves travel any great distance, but that they transmit this kind of motion; and thus we obtain the sort of action which is termed the wave. Now, in order to know all about a wave, we must in the first place know the wave length: by that we mean the distance from crest to crest. Here I have the highest point in this wave, and here I have the highest point in the next wave, and the distance from crest to crest is termed the wave length. This wave length varies materially. In the little waves on the pond the wave length will be but small; in the gigantic billows of the ocean the length from crest to crest may be so great that the length of the ship may be contained in the trough of the wave, and that persons on board are not able to see over the top of the two wave crests in front and behind. So long is the wave length in the case of these large oceanic billows. Besides this wave length, we likewise require to know (1) in what direction, as regards the path of the waves, does the vibration of the particles occur; and (2) what is the amount of that vibration, or how much are the particles disturbed. If we know these three things we know all about the wave.

Now, let us try to find out whether there are any other kinds of wave-motion with which we are not so familiar as with those of the motion of particles of water. There is the peculiar motion of the particles of air which we term *sound*. I will show you, in a somewhat peculiar way, the transmission of sound. [A sharp

explosion was heard at the opposite end of the hall.] Let us ask ourselves, what is the mode in which this sound is transmitted to our ears? In this case we must call science to our aid; and science enables us to see and understand those invisible motions of the air which produce, upon the tympanum of our ears, what we term sound. We know as certainly the laws which regulate the transmission of sound through the air, and through other media, as we know the laws affecting the phenomena of the transmission of the wave-motion in water. That the same thing holds good here with regard to sound as with regard to water I think I can show you by means of this arrangement. I have a long glass tube here, into which I will blow a small quantity of smoke. You see I have blown a small quantity of smoke into one end. A lighted candle is placed opposite the other end of the tube, and I want to show you that I can produce a wave of air which will blow the candle out, without sending this smoke through the tube. I have no doubt that when I simply knock these two boards together, I shall be able to blow out the light at the other end of the tube, without this smoke travelling along. You see that the candle is blown out by the wave of air, and without sending the smoke through the tube. And thus, by this experiment, you learn that it was really a wave of sound that blew the candle out; that the particles of the air did not travel from end to end of the tube, but that there was only a contraction of the air in one place, and a rarefaction of the air in another place; and this effect, or wave travelling along, blew the candle out. Sound, then, is produced by the alternate condensation and rarefaction of a column of air; and it has been found that sound travels at the rate of about eleven hundred feet per second, at the ordinary temperature of the air. We can tell that sound travels but slowly, in comparison with light, by looking at artillery practice or rifle shooting at a distance: you can see the flash of the guns or the rifles long before you hear the report. And you may in this way calculate the speed of sound, if you know the distance of the artillery, by observing how long the sound takes in reaching you. Or, if you do not know the distance, but do know the rate at which sound travels, then you may calculate the distance the artillerymen are from you.

Sound can also be reflected, as in the case of the echo. The echo is nothing more than the reflection of the waves of air, producing sound. So, too, in the whispering gallery of St. Paul's Cathedral, in London. You may speak in the gallery at one end in a low whisper, and at the opposite end of the diameter of the

cupola you can hear what is said distinctly, although it is only whispered at the other side. This is due to the fact that the waves of sound can be reflected.

Another thing that shows that sound has to do with our ear, and is due to the vibration or waves in the air, is the fact that sound cannot pass through a space which is free from air. If you exhaust or take away all the air contained in any vessel, and set a bell ringing inside this vessel, you will not hear it, because sound affects the drum of the ear alone through the vibration of the air, and unless the air can be set into vibration we do not hear any sound.

Now, then, what are musical sounds? We all know the difference between a noise and a musical note. A musical note is nothing more than a very quick succession of sounds or distinct impulses given to the air. If I have a toothed wheel, and have the means of revolving that toothed wheel rapidly, and then bring a piece of wood against the teeth of that wheel as it is revolving, you will hear, when you first begin to make it revolve, a succession of distinct raps or noises; but, as the speed of the wheel increases, you will begin to hear a deep hum, or low bass note; and, gradually, as you increase still further the rate at which the wheel revolves, the stick will be struck quicker and quicker, by the teeth of the wheel, and you will then get a succession of musical notes, each one becoming more treble, or higher in pitch, as the number of impulses which are thus given by the teeth of the wheel become greater and greater. Now, it is well known that the lowest number of these impulses which give rise to musical notes is sixteen in one second, and that speed gives rise, not to a succession of noises or beats, but to a regular bass note; whilst the very highest note which is audible, and can be perceived by the ear, is caused by about 38,000 in a second of these distinct knocks or beats. So that from sixteen in one second to 38,000 in one second we have a succession of notes; and we come to the conclusion that the ear is capable of hearing about eleven octaves; that is to say, the vibrations are doubled about eleven times from sixteen to 38,000. So much for the vibrations of the air, which we term "sound."

We now pass further to the next point, and we ask: Are there any other kinds of vibrations that science has to tell us about? Are there any other kinds of waves which produce effects different from those which we have observed in sound? There are. Science can tell us of some other kinds of wave-motion which produce effects wholly and entirely different from either the waves

of the sea or from the waves of the air as exhibited in sound. What are these? How does the light of the sun reach us? It can be proved by astronomical observation that the light of the sun does not reach us instantaneously—that it takes some time for the light of the sun to travel the distance between the earth and the sun, about eighty-nine or ninety-two millions of miles. This fact has been observed by means which I am afraid I cannot fully explain to you now for want of time, by what is called the occultation of the satellites of Jupiter. The astronomers, so accurate and so perfect is their science, can calculate exactly the time when Jupiter's moons will disappear either in front of or behind the planet Jupiter. They find, also, that Jupiter is at one time further from the earth than at another time. For the planets revolve round the sun, and you can imagine that the sun at one time may be between us and Jupiter, whilst at another time we may be on the same side of the sun as Jupiter. Hence the distance from the earth to Jupiter will vary according to the relative positions of the earth and this planet. Well, now, I say that the astronomers find that, according as the distance is less or greater from the earth to Jupiter, the time at which this occultation or disappearance of the moons of Jupiter takes place varies, and that the nearer we are to the planet the nearer does the time come up to what the calculation shows; whereas the further we are apart the more does the observed time of the occultation lag behind the calculated time. And thus we come to the conclusion that this difference is really due to the time which the light takes in travelling to us from Jupiter. It has thus been ascertained that light travels at the enormously rapid rate of about 192,000 miles in a second. I may just by the way remark that the rate at which light travels can be measured on the earth by a very ingenious arrangement, which I cannot now describe. It has also been found that if we observe very accurately the time which light takes to travel from the observer to a mirror placed a mile off, and back again to his eye, that this does not take place instantaneously, but that a certain interval of time has to elapse in order that the light shall travel that distance; and we may remark that the rate of its passage, as thus found, agrees with the results of the calculation by the astronomical method of observation which I described, namely, the occultation of the satellites of Jupiter.

How does this light travel? At one time it was thought that light actually consisted of light-matter, or some material substance which was shot out from the sun with this enormous rapidity, and

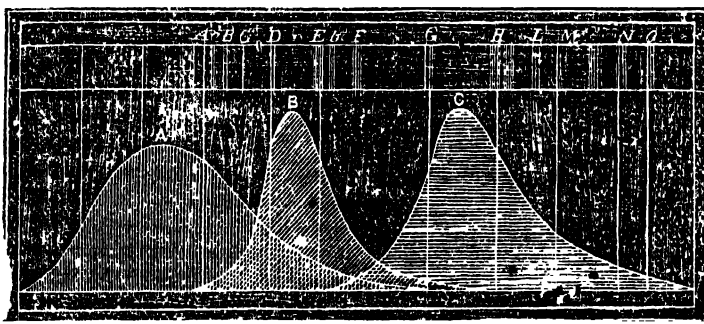
impinging itself upon our retina, produced the effect which we term light. That was Newton's idea. But since Newton's time certain facts have come to the knowledge of men which were not known during Newton's life, and which have shown us that his supposition will not explain the matter; and we must seek some other explanation of the mode in which light is propagated. Now I say that light is propagated in a similar kind of way to sound, that is, by a kind of wave-motion; and it is with regard to the kind of wave-motion with which light is propagated, and of some of the connected phenomena, that I wish especially to speak to you to-night.

Now, in the first place, the waves of light are enormously more rapid than the waves of sound. I told you that the greatest number of sounds which can be heard by the ear, or that produce an audible effect upon the ear, is about 38,000 in a second. If we get up a greater velocity than this we lose all sense of noise; beyond this the human ear is not capable of receiving an impression, and entire silence ensues. Now imagine, if you please, that the vibrations continue. Let us suppose ourselves in a perfectly dark room, and let us imagine, first, a series of vibrations which give us distinct raps; that these raps increase in number until we get a bass note; that gradually as they increase the note becomes shriller and shriller, and at last we reach the number of 38,000 in a second. Then the next increase of speed ceases to produce any impression upon our ears. Let us still imagine that the vibrations are continued, and become more and more rapid, vastly more and more rapid, so that, instead of having thousands in a second, we come to have millions in a second. We cannot mechanically produce a number of vibrations, but we can in another way, as I will show, produce such rapid vibrations. Imagine these millions of vibrations in a second. What would you observe? You would notice, first, that a pleasing warmth began to diffuse itself from the vibrating body; then a red light would begin to exhibit itself, and this light would become more visible as the velocity of the vibrations increased—it would become more yellow, and at last the vibrating body would become white hot. Thus you see that as we increase the rapidity of the vibrations perfectly new phenomena become apparent. First we have the phenomena of heat; and as we further increase the number of vibrations we get the peculiar phenomena we know as light.

Now I have here a diagram giving you the wave lengths of the heating rays, of the lighting rays, and of other rays which, although

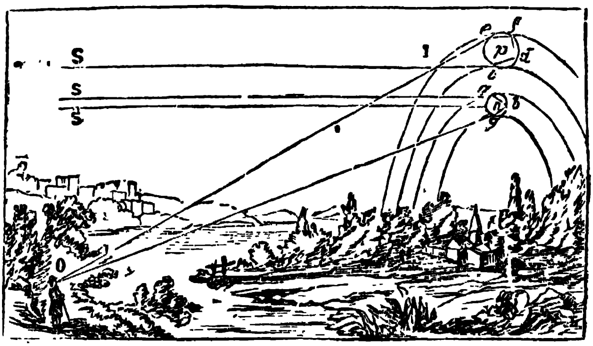
they give off but very little heat and light, are yet existent, and are capable of producing, as we shall see, some very remarkable effects.

I have told you that sound can be reflected; and that in the common echo, and the whispering gallery, we have illustrations of what is called the "reflection of sound." So, too, you know very well that we can reflect light; and that if you take a bit of common looking-glass, you can, when the sun shines upon it, reflect the sunlight about, with this bit of glass, pretty much as you please.



Light can, however, not only be *reflected*, but it can be *refracted* or bent out of its course. Here, for instance, if I take this bright ... light of the electric lamp, I can reflect the light about on the walls and ceiling of the room, by means of this little mirror, and make it dance in all directions. This is the reflection of light. I say, also, that light can be refracted; and you can see here a beautiful coloured band of light which has been thrown on the screen. This beautiful coloured band is due to the refraction of light. It was Newton who first pointed out this power of white light to be separated into differently-coloured rays. Here you see the coloured band passing through the intermediate stages, from red to blue or violet. Now the whole of these differently-coloured rays consist of waves of different degrees of length, as you see in the table. This shows us that the red ray vibrates about two hundred millions of millions every second: whereas, the blue ray vibrates about seven hundred millions of millions each second: and thus, you get an idea of the enormous rapidity with which light travels, and with which the vibrations of light occur. So much, then, for the refraction of light.

Now this splendid parti-coloured band of light was first seen when the first rainbow was seen; and the first rainbow was seen—or might have been seen if there had been anybody there to see it—when the first sunshiny shower fell on this earth. For the rainbow was produced then, as it is produced now, both by the *reflection* and by the REFRACTION of the waves of light on the drops of water falling down as rain. Now the true explanation of the different colours of the rainbow was first given by Sir Isaac Newton, in the year 1675; for he showed that lights which differ in colour differ also in refrangibility, and that the whiteness of light is caused by its being compounded of all the elementary colours mixed together in due proportions.



I cannot now enter fully into the optical construction—if I may use the word—of the rainbow. I will, however, show you a very rough diagram or painting of a rainbow; for who indeed can paint a rainbow? And I will indicate to you its mode of formation.

In the first place I want you to notice that the rainbow is always seen opposite to the sun. You never see a rainbow excepting when you stand with your back to the sun. Very well; then I say next that when two bows are seen, the first rainbow is due to *one* reflection and *two* reflections of the rays of light in the drops of water; whilst the second rainbow is due to *two* reflections and *two* refractions of the light within the drops of water. And there is no reason why we should not see three or four, or an infinite number of rainbows, if the human eye was sufficiently sensitive to light and colour. The mode in which the rays of light are reflected and refracted in the drops of water is shown in the

figure. The horizontal sunbeam strikes the drops at *n* and *p*; the single reflection from the drops *n* gives rise to the first bow, whilst two reflections from the drops *p* causes the second bow.

Now the rainbow is seen at the highest when the sun is lowest; and when the sun is a certain height above the horizon the rainbow is not seen at all. When the sun is low the bow is high; and when the sun is high the bow is low. When the sun's elevation is above 42 degrees and 30 minutes, then on the plain you will see no rainbow at all; but if you go up the mast of a ship or up a tower, you may then see the rainbow, when you cannot see it on the surface of the ground; in fact, from a height, such as the mast of a ship, where you get a complete horizon all round, you can see the rainbow perfectly circular. The colours of the rainbow then arise from refraction and reflection of the light, exactly in the same way as I have shown you on the screen the beautiful coloured band of light of the solar spectrum, produced by passing the white light of the electric arc through a prism. And as the first bow is caused by one reflection and one refraction, the red (being that part of the light), which is *least* bent out of its course, is on the *outside* of the bow; whereas in the second bow (caused by two reflections and refractions) the red is on the *inside* of the bow.

Now, then, let us examine this bright band of light which we have in the rainbow, and which was explained to us first by Newton. Let us see what science tells us about the different parts of this coloured band. Do we find all the different parts of the coloured band possessing the same properties, or do they vary? These are the questions which now, with your permission, I will endeavour to answer. And I will try to show you what I may term the lessons of the rainbow, this glorious coloured band which we see spanning the heavens. Now Mr. Harrison will first be kind enough to show us a diagram to illustrate the differences of colours observed in the different parts of the spectrum, and then I will show you some experiments to illustrate the explanations I have to give. Here I have got what you will be good enough to imagine is a spectrum, that is to say, a coloured band of light. Here, on the left of the figure, we have the red part, which you saw in that coloured band on the screen. Now I want to describe to you what is found in the red part, what we find in the yellow and green part, and what we find in the blue of this coloured band. I want to show you that in the red part of the band there is a preponderance of a certain class of waves or rays which are termed the "*heating rays*;" that in the yellow part we have a

preponderance of those kind of waves which are termed the "*light rays*," those which produce the effect of light upon the eye; and further, that in the blue portions of this band we have a new set of effects come into play, produced by what are termed the "*chemically active rays*." These blue portions are incapable of giving much light or heat, but they are capable of doing a different kind of work, namely, producing what we term "chemical action" or chemical change. Now, in order to render this quite plain to you, let us again consult the diagram.

This diagram is intended to indicate to you this division of the spectrum into three particular kinds of actions—the heat actions, the light actions, and the chemical actions. Here now is the red end of the spectrum: this is the part which is signified by the letter A. We pass on and come to the yellow part of the spectrum. We pass on through the green and come into the blue part of the spectrum; and, lastly, we pass into the violet portion, which is scarcely visible to the naked eye. Now let us return to the red portion. This curious curve which rises here, and which is shaded by these vertical lines, indicates to you the *heat* of the spectrum. This mountain which rises here shows us the part of the spectrum where there is most heat, and where the thermometer would rise the highest. But you will notice, too, that here there is another mountain (Curve B) and a much steeper one, with these slanting lines upon it; and this mountain indicates the intensity of the visible portion of light, that which affects the human eye. You will notice that that is most visible about the yellow part of the spectrum. Here we have a third mountain (Curve C), rising up and tapering down, and this mountain represents the amount of chemically active rays; and here where the mountain rises highest you have the maximum of chemical effect.

Now we go back again to the heat. Let us remember—heat, light, and chemical action. Where does the maximum of heat occur? Why, it really occurs a little beyond where the spectrum first begins to be visible; that is to say, here is the place where the spectrum is first seen. You will observe that the top of the heat mountain is to the left of that, in the invisible rays—those rays which I told you are not capable of producing an effect upon the eye, but are capable of producing the effects of heat. Very well, now I will show you in a few moments that this maximum of the heat rays is beyond the visible rays; the maximum of light rays is in the yellow; and the maximum of the chemically active rays is in the blue or violet. In the red rays there is no chemical action; in the blue rays we have little or no heat; whilst

both in the blue and red we have not much light, the light being chiefly produced in the yellow rays.

I will now proceed to illustrate these three kinds of action which we find in the solar spectrum, or in the spectrum of the electric arc. I will first show you the light effect of the yellow rays which strike the eye most intensely. I have here the means of producing a very bright yellow flame. Now, you see that it is a flame of an intensely yellow colour. [A placard was suspended, with variously-coloured letters upon it, forming the words "monochromatic light."] I first show the letters in their true colours; but, if I increase the yellowness of this flame, the effect of the yellow light is to neutralise the other colours, and produce a monotonous and ghastly yellow appearance. Now, that indicates to you the effect of the yellow kind of light— the light of one degree of refrangibility; and, it is this yellow light in the sun, and in almost all artificial lights, which mainly affect the eye.

Next I want to show you that if I even cut off all the rays of light which pass through this electric lamp— for I can "filter off" all the rays of light—yet I shall have sufficient quantity of the invisible rays of heat present to produce the effect of ignition. Here we have what Dr. Tyndall properly termed his "ray filter," by means of which I can keep back from the light the visible rays, but which will allow the invisible heat rays to pass; so that now, if I let the electric light pass through, I shall have a perfectly dark focus; in which, however, I shall be able to set fire to some paper. Now you can see nothing coming out from the front of the lamp, though the light is burning brightly; but if I bring a piece of paper here I shall be able in a moment or two to produce heat sufficient to ignite the paper. There it is, burning! You see that a piece of black paper has been inflamed in the focus of the invisible rays, and thus we learn that we can take away all the visible emanations, and that a large number of heating rays will still remain.

Let us pass on to the next most refrangible portion—the blue rays of the spectrum; and let me, in the first place, show you an experiment to prove that we may cut off all the red or heating rays and the yellow rays, and almost all the visible blue rays, and yet that we shall have beyond this a certain number of rays which have the power of producing chemical action. I want to show you that we can produce chemical decomposition when we have cut off all the rays except the ones which have passed the blue. I shall here use the electric light again, and I will place a number of blue glasses before the apparatus. You see that very little light

comes through—only the pale violet rays. Now I am going to place in that focus—not a piece of paper covered with a little gunpowder, as I did before, but a little glass bulb which contains two gases, chlorine and hydrogen, which have the power of combining together when they are exposed to the light, which contains large quantities of these very rapidly vibrating blue rays. I put this little glass into this blue focus; and now, Mr. Harrison, be good enough to turn the light on. Again you see we have to wait for the effect, but we shall get it in a moment. [Loud explosion.] There! you notice we have a little explosion. This shows you, then, that when we have cut off nearly all the heating rays, and nearly all the lighting rays, we still have something left, namely, those rays which are capable of producing this change; for, as I will show you, the red rays will not produce this change. I have at the other end of the hall placed a concave mirror, and I have here the power of throwing a parallel beam of electric light on to that mirror. There you see we have our beam, and there are the beautiful carbon points which give the light. Now, Mr. Harrison, be so kind as to bring the image of these carbon points on to our mirror down below, and then Mr. Heywood will arrange the focus, and then I will show you that we can get, as we did before, a little bulb exploded by this reflection of the chemically active rays. Thus, then, you see that the chemically active rays obey the same laws which the visible light rays obey. Now Mr. Heywood will place the bulb in the focus, and then, as soon as Mr. Harrison brings his points together, we shall have a sharp explosion. [The explosion followed instantly.] That is due to the reflection of the chemically active rays. Now we will put another bulb there, but between the light and the mirror I will hold a piece of this red glass. And why is this glass red? Because it does not allow anything but red light to pass through it: it does not allow any blue light, or any of these chemically active rays, to pass. Hence, if I put a red glass in the path of the luminous beam, we shall not have any explosion of our bulb. You see now there is no explosion, because I am taking away or filtering out all the chemically active rays by means of this red glass. Next I will take away the red glass and put in a piece of blue glass. The blue glass is blue because it allows the blue rays to pass, and because the red rays cannot pass; hence the chemically active rays will pass through, and we shall get, as I imagine, an instantaneous explosion. I will do it again in this way: I will first take both glasses, red and blue, and then we shall have no explosion; then I will take away the red and you will get an explosion. There is no explosion now, for you

see the red light shining on the mirror. Now I will take away the red glass. [Explosion.] Instantly you observe the effects.

Now there are a variety of ways in which I might show this fact—that we have at one end of the spectrum these peculiar blue rays. I will, however, as we have got the bulbs there, show you one other experiment which is very characteristic and very interesting. I have here two glass globes. One of these globes is filled with a gas which we know as chlorine gas, and it appears by gaslight nearly perfectly colourless. By gaslight you cannot tell which globe contains the yellow gas, and which contains common air. If Mr. Harrison will be good enough to give us a beam, you will see the difference. This is the globe which is filled with air, and this globe is filled with chlorine gas. If I place this between the light and the mirror, the whole of the chemically active rays will be taken away by this chlorine gas, which absorbs or takes up the vibrations; whereas if I put the other globe filled with common air between, we shall find that the explosion takes place instantly. [Explosion.] If it were not tiring you I might illustrate this fact further. I might show you how I can produce the effect by burning a piece of magnesium. Here I have some little powders of magnesium; and if I place one of these bulbs near these bright flashes, I shall obtain the little explosion which you have noticed before. I will burn one of these magnesium flashes, and the light which this produces will be sufficient to explode the bulb, which will be shattered by the chemical union of these two gases, chlorine and hydrogen. These two gases will unite together. There you see the bright flash, and you hear the explosion. This explosion, let me explain, is due to the sudden combination of the chlorine and hydrogen, producing a substance which we know as hydrochloric acid. Now then, between the magnesium flash and the little bulb I will insert this piece of red glass, which, as you have seen, has this power of sifting out the whole of the chemically active light, thus preventing our little bulb from exploding. We shall now get no explosion when the light of the magnesium flash passes through the red glass. You see the flash is gone off, but there is no explosion. Now I will substitute for the red piece a blue piece of glass, and the same thing will occur. [Explosion.] You see that the chemically active rays can pass through the blue glass, and the bulb has disappeared—it has been shattered into ten thousand fragments.

Thus we have learned that the rainbow is not only a beautiful and striking object, which we all admire when we see it in the heavens, but we have learned something about the rainbow which

perhaps we did not all of us know before—we have learned something concerning the properties of these different coloured lights which make up the rainbow. We have seen that at one end, namely, the red end of the coloured band or spectrum—for the rainbow is made up of an infinite number of straight little bands, from red to blue, making altogether the glorious arch which we see in the heavens—I say we have learned that at the red of this coloured band, the special effects of which are for heating, that there the waves are slower, that they do not vibrate so quickly as in the other parts of the spectrum; that as we pass on to the yellow wave, producing what we know as the luminous effects, that there the rays vibrate more quickly than the red, but not so quickly as the further portions in the green or in the blue; that, thirdly, as we come into the blue, we find a different class of actions setting up—we find that there we have to do with rays which possess peculiar powers, which although they have little or no heating power, and little power of affecting the eye, yet have the power of producing those chemical changes of which, in fact, the beautiful art of photography is only one. It is by these blue rays that the photographer is able to make his picture; and if I had time I might show you that we can photograph only in the blue whilst we cannot photograph in the red ray, because it is in the blue alone we have those special rays which are capable of producing the disunion or disruption of the chemical compounds—those chemical changes which the photographer makes use of on his sensitive plate.

Well, then, we have these three distinct kinds of action; but I don't wish you to go away with the idea that there are in the different parts of the spectrum any absolutely different kinds of radiation: the heat rays, and light rays, and chemically active rays only differ from one another in the number of their vibrations and in the length of their waves. You have in one column of that diagram the wave lengths given, and in the next column the number of vibrations; therefore, each of these differs in wave length, but it differs in wave length only: that is to say, we have not a distinct kind of ray which we term "light," and another distinct kind of ray which we term "heat;" they are all the same kind of vibration, only differing one from another in intensity and in length of vibration, or in length of wave.

I hope, even in the short time we can devote to a subject of this kind, that I have made these three chief points clear to you. I would only remind you, moreover, that this is by no means the only story which the rainbow tells—that this is but a very small part

of that which science teaches us about the rainbow; for you will well remember the lectures, I am sure, or at least those who were present at them when given in this Hall, on Spectrum Analysis, by Dr. Huggins and Mr. Lockyer and others, who showed that we had thereby gained immense knowledge concerning the chemical composition of the earth, the sun, and even the far distant stars. I only remind you of this because the examination of the rainbow is only one branch of spectrum analysis. This evening I have endeavoured simply to bring before you the general arrangement of these vibrations in the spectrum, so that you might get some idea of what is meant by a wave—first what is meant by a wave in the water, what is meant by a wave of sound, and then what is meant by a wave of light—and thus get some notion of these waves in the rainbow.

THE ICE AGE IN BRITAIN.

A LECTURE

BY PROFESSOR GEIKIE, F.R.S.,

Delivered in the Hulme Town Hall, Manchester, November 6th, 1872.

SCATTERED over the northern half of our islands, from the Pentland Firth far south into the heart of England, there lie innumerable blocks of stone which, if you look at them with some attention, you find to be unlike any other rocks, either of the hills or of the valleys among which they lie. In Switzerland, where similar blocks occur, they are called by the very happy name of "foundlings," for they tell us little or nothing of their parentage. Blocks of granite, for example, have been strewed over the hills and valleys of Yorkshire; and yet, as you are probably well aware, no granite exists anywhere in their neighbourhood. These travelled stones occur of all sizes, sometimes as big as a cottage, and they lie sometimes in the oddest places, perched it may be on the edge of a ravine, or in the bottom of a valley, or even on the top of a high hill. But besides these blocks, over the same regions run strange mounds of sand and gravel, sometimes for miles along the country. They wear such an artificial look that one can hardly help at first sight regarding them as parts of artificial ramparts. In some districts they have a green, grassy surface, which contrasts strongly with the dark brown heath across which they run, and serves to mark them out, even at a distance, from the surrounding scenery.

In addition to the travelled stones and the winding ridges of sand and gravel, a great deposit of clay and stones lies spread all over the centre and north of England, over Scotland and Ireland. You are no doubt familiar with sections of that clay cut in the neighbourhood of Manchester. You would usually have to sink through it before coming down to the solid rock. The

clay is stuck full of stones without any order. Look at these stones, and you will find that they, too, partake of the "foundling" character to which I have referred in the case of the large scattered boulders. They belong, in many cases, to rocks which you could not find anywhere in the neighbourhood.

Now these surface features are characteristic of the whole of that wide region of the island to which I have referred. They evidently furnish proofs of some very powerful force, whatever it was, by which such blocks of stone were moved and dropped on their places, by which these great mounds and ridges of gravel and sand were heaped up, and by which these enormous deposits of clay were spread so far and wide over the country as we now find them.

You can easily see that features of the landscape so striking as these, more especially those "foundlings," or "erratic blocks," as they are called by geologists, could hardly fail to arrest the notice of our early ancestors. Even in ancient times they must have appeared too singular to be referred to any known or visible agency. Hence long before science arose to supply any explanation of them they gave rise to abundant legends and superstitious beliefs which have come down to our own time. Around these features there has gathered a sort of special mythology—stories of old wizards, warlocks, brownies, fairies, and even of the arch enemy of mankind himself. For instance, in some parts of the country men still point to those green, grassy mounds as the work of fairy hands, and as having often received into their bosoms the hapless wight who fell asleep upon their surface, and who, after having slept seven years in fairy land, awoke to find himself upon the self-same spot. In other districts where the large stones are abundant, we find traditions having reference more especially to the devil, who seems to have employed himself in playing sometimes at bowls, sometimes at quoits, or at the Highland game of putting stones, and the big boulders are pointed to as the implements which he used for his diversion. In many districts, too, even the more odd-shaped forms of the rocky surface suggested to the grim humour of our ancestors the name of "Devil's Punch-bowls"—a name so common as to show that he was credited with a love for the convivial habits of old times, in almost every part of the country. And, lastly, certain ridges are still pointed to as strands of the rope which, to keep him from further evil, some mischievous spirit was employed to twist out of sand, which, baffling his attempts, fell from his hands and has remained there in ridges ever since.

When science began to look at these surface features and tried to find some explanation of them, they were still almost as puzzling as they had been in the days of legends, since there was so little in the country to throw any light upon them. Among the earlier explanations, the Deluge furnished a convenient solution of the difficulty—a solution which found the readier acceptance since it seemed to bring confirmation to the Biblical record from the records of geology. The Deluge was appealed to with confidence as the originator of the great sheets of clay that cover the country, as having piled up the ridges of gravel which run far up the sides of the hills and along the bottoms of the valleys; and as having also driven the great blocks of stone for leagues across the country, and perched them even on the tops of the hills. By and by, however, men found that the Deluge would in nowise serve as a tenable explanation. Reference was then had to "earthquake waves." Men supposed that in some region of the Atlantic some great subterranean convulsion had occurred, whereby vast waves were driven with enormous force upon the land, that they rolled over it from the west or north-west, and that in their passage they produced the various surface features which we have noticed. But in the end it was found that this explanation also would not do.

Now this evening I would propose that you and I, as if we knew nothing whatever about any previous explanations of these singular characteristics of the surface of our country, should pass them in review before us, and see how far we can find an explanation for ourselves; in so doing we shall in some measure go through the same mental process which has occupied the geologists of this country during the last thirty years; and the task may help to give you some notion how such scientific questions are worked out.

Let us begin by stripping off in imagination all those clays, sands, gravels, and boulders with which the surface of the country is strewed, till we get down to the solid rock. Well, what do we find underneath them? We find that wherever the rock has been hard enough to retain the marking, it has been ground down, polished, grooved, and scratched. Let us look at one of these rock-surfaces. If hard and solid, like sandstone, it has been ground away till it has acquired a smooth, polished face, over which we see innumerable scratches, as fine as might be produced by the point of a diamond, ruts as coarse as those of a carriage wheel, and deep grooves in which a human body might easily lie. Such markings are probably not like anything you remember to have

noticed before. You observe that the striæ and grooves all run in one general direction on each sheet of rock which you examine. They are not mere hap-hazard, random markings. Moreover they are present almost everywhere below the clay when you get compact rock underneath it. Even when the clay has been for some time removed you may often find them not yet effaced. Throughout a great part of the country, however, the clay has been washed away, and the rock, exposed to the crumbling effects of the weather, has lost the finer scratches, yet the general smoothing of the surface remains still.

You will have some notion of the general effect of these smoothed rocks on the landscape if you turn to one of the diagrams on the wall, which represents the top of one of the hills where this characteristic contour is most conspicuously seen. You will notice that in this drawing there is a smoothing of the top of the hill from the left hand to the right, that all the little scarps of rock which face upwards to the right hand have a sharp truncated edge, and that all the ledges which slope to the left down the hill are smooth and ground away. Now were you to climb that hill-side, you would find the hard rock to wear exactly the characters which I have just described. The whole surface has been smoothed and ground away; and though the finer scratches and grooves have faded from the long exposed ledges, you may still find them by peeling off some protecting layer of turf or patch of clay. Moreover, you would see that these lines, though they sometimes cross, yet all have a general direction which corresponds with that in which the smoothed rock surfaces run. If from that hill you passed on to other hills in the neighbourhood, you would find them to be similarly ground smooth and scratched. In the course of your observations you would in the end discover that the scratches and grooves diverge from the main masses of high ground in the country, and radiate outwards and downwards to the sea. Thus from the Grampians the striæ go away out east, west, south, and north into the plains, and even cross considerable intervening ridges. In the uplands of the south of Scotland the same thing takes place: these high grounds give rise to another set of rock markings, which on the one side strike northward to meet those which are coming south from the Highland hills, and on the other go southward to meet those which are coming down from high grounds in the north of England. Cumberland and Wales show the same facts. Each great area of high ground in the British Islands is a centre from which these markings diverge in all directions.

This striation of the solid rocks is one of the first features to arrest our attention. But even where the finer markings have been effaced—as they invariably must be effaced by the gradual wasting to which the whole surface of the earth is exposed—the eye that has been trained can still detect the worn, smooth, and polished surface which is represented in that diagram.

But, in the second place, these striæ not only diverge from the high grounds—they have totally disregarded inequalities in their course, very much as a river disregards the pebbles that happen to lie along its bottom. If you watch the channel of a stream, you will find that if there happens to be a big rock in its course the stream mounts up over the rock and descends on the other side. Well, whatever the agent was which produced these scratches on the rock, it seems to have moved in sufficient mass to completely envelope and overflow obstacles in its path, for you find that the striæ not only diverge from high grounds, but mount up and over minor but still conspicuous ridges. The same diagram to which I have pointed furnishes an illustration of this fact. That is a tolerably high hill in the Island of Bute, lying in front of the higher hills of the Highlands, and yet the striæ and ruts upon the rock ascend from one side, and go completely over and down the other side.

A third fact comes very impressively before us when, peeling off the clay, which has acted as an admirable preserver of these fine sculptures, we get down to the fresh surface of the rock. We then find that the finer striæ go into all the little hollows and rise out of them. Each dimple of the rock is scratched with fine lines in the same way that the rock on a great scale is scratched. So you see that the agent, whatever it was, which ground away the rocks, not only diverged from the high grounds and mounted up hills, but was able to insinuate itself in all the little creeks and crannies of the rocks, completely moulding itself upon and grinding down the rock-surface over which it moved.

Now, what possible agent was it which effected this sculpturing of the rocks of this country? Let us see if anything goes on around us now that will help to explain it. We first try the sea shore. You examine the rocks along any part of the coast line of Britain—take even as rocky a part as you choose, such, for example, as where the huge swell of the Atlantic bursts upon the land, and see if you can find there anything at all analogous to these rock-sculptures which I have described. You will find nothing at all analogous. Now and again, where a steep cliff meets the waves, large caves and inlets have been drilled into the

rock by what has been aptly called a kind of "sea artillery"--the waves catch up big blocks, swing them forward, and, dashing them against the base of the cliff, hollow out great tunnels in the solid rock. You may note at the ends of those tunnels that the rocks are sometimes battered and covered with bruise-marks by the stones, but no scratches appear; the sides are well smoothed and polished, but there is nothing at all resembling our abundant striæ and grooves. Next try a river channel. Take any river, the quickest or the slowest in its flow. Examine its channel where it sweeps over the rocks. These you find to be sometimes tolerably well polished; but there is no persistent scratching along their surface. Now and then, indeed, a stone pushed across them leaves a mark, but nothing can there be seen of the same general kind as that widely diffused rock-sculpture which we have been tracing elsewhere. Besides, we cannot for an instant suppose that markings found all over the country, on the tops of the hills as well as in the bottom of the valleys, could by any possibility have been formed by anything like river action.

Well, you bethink yourselves of still one possible explanation. You might suppose, as was done by some early observers, that these marks were made by great floods or sheets of water launched across the country, owing to some convulsion under the bed of the Atlantic. We can occasionally put such a supposed cause as this to the test: when, as unfortunately sometimes happens, a great reservoir bursts, and suddenly discharges an enormous volume of water down a valley, we can watch what happens to the rocks over which all the stones and mud and gravel are hurried. There, if anywhere, we should find something like our rock sculpture if it be due to the action of violent floods. We meet, however, with nothing at all approaching it. Here and there the rocks may be scored and bruised, but with complete irregularity. The flood of water acts in a much more tumultuous way than the agent we are in search of, which polished and scratched each little dimple and crevice with as much leisurely care as it bestowed upon the moulding of a broad valley-bottom or a great mountain-slope. We have exhausted all the possible explanations which offer themselves here, and evidently we must go beyond Britain if we are to find anything in living nature which will throw light upon the way in which the hills and valleys in Scotland and the north of England and Wales have been polished and scratched.

It is now about thirty years since a distinguished naturalist came to this country fresh from the snow-fields and the glaciers of Switzerland. To his own delight and the surprise of people

here, he recognised these puzzling rock dressings as identical with those of his own country, which he knew to have been done by the glaciers. In the year 1840 he published an account of what he had seen in Britain, and called the attention of British geologists to it. Yet the facts were so strange, and his explanation of them seemed so far-fetched, that for many years his remarks were allowed to lie dormant. Now, however, they are very generally accepted.

It was long after he had made known what was the true explanation that I resolved to compare for myself the rock-sculpturing of this country with that of a country where glaciers still exist. For this purpose I selected Norway, not that I hoped actually to make the demonstration clearer than it had now been made, but I wanted to see with my own eyes how far it was possible to connect the rock markings of past time with the same kind of work going on to-day. Now, let me give you an account of what I saw, and which will be made more intelligible by one or two diagrams of parts of the Norwegian coast-line and its glaciers, which are made from sketches I took on the spot.

If you were to pass from the part of the country where we are now assembled to the north-west of Scotland, you would find yourselves in a very rough, bare, mountainous region: you would meet with rocks having but little covering of clay and gravel upon them, but standing up in bare ledges and rugged slopes, and having very conspicuously marked upon them that general smoothed contour to which I have referred. The whole of that coast, running from the outer islands far away up into the heart of the glens, reveals this characteristic form of surface. I was already familiar with that region, and I felt that but for the restless surface of the North Sea, which makes itself only too memorable an experience, one might have supposed himself to have fallen asleep on the west coast of Scotland, and to have awakened again in the same place. Landing upon the coast of Norway you have precisely the scenery of the west of Scotland over again. Even upon the map this is shown in a tolerably distinct way. You can connect the backbone of Britain with the backbone of Norway. You see how indented the west coast of Britain is with those long arms of the sea, or fiords, and how on the west coast of Norway precisely the same kind of physical geography obtains. Well, I found, as I had anticipated, that on the outer islands of the Norwegian coast the same smooth polished contour marked the general scenery; that the same abundant evidence of striation could be traced wherever the rocks were examined; that the

striæ moulded themselves among the dimples of the rock; that they went up over the hills, and that they diverged from the high grounds exactly as they do at home. I followed these rock-markings up one of the fiords which promised to yield the best results. I found that they wound in and out with all the windings of the valley, that they moulded themselves completely to the rock. I might have supposed I was tracing them up one of the sea-lochs of Inverness-shire. Tracking them step by step, at last I found them passing underneath the glacier, that is to say, a great tongue of ice coming down from one of the broad Scandinavian snow-fields. I crept underneath the blue ice and found bits of stone locked fast between the ice and the rock. It was easy to see how the sharp points of these stones made the scratches on the rock, and how the grains of sand and mud, pressed onward upon this pavement on which the ice was slowly sliding, produced that smoothed, polished surface so conspicuous wherever the ice had reached.

I caught the ice, as it were, in the very act of doing the work of which I was in search of some proof. I shall never forget the delight with which these facts first burst upon me. I found that the glazier was retreating—that only a year or two before, the sides of the valley, which were now bare rock, had been covered with the ice, and that the striation and rock-polishing could thus be seen in almost every stage of development—some only just beginning to form, as it were, under the ice, some graven perhaps only a year or two before; while far in the outer part of the fiord some probably were quite as ancient as parts of the striation in our own country.

Such scenes as that in Norway tend to bring very vividly before us the conditions which must have obtained in our own country during this great period of ice-polishing. The land must have been covered with one vast sheet of ice very much like Greenland—and down from all the main valleys out to the sea went large tongues of ice or glaciers, from the ends of which fragments broke off as icebergs and drifted away, covering all the sea with fragments of ice.

Let me point to one or two features of that Scandinavian coast-line depicted on the diagrams, which may enable you to realise more clearly than words can do some of the features which characterised this ice age in our own country. In that upper diagram, for instance, which represents the side of one of the Norwegian fiords, you will notice that the mountains all range up approximately to one general level; that, in fact, the side of the

fiord is the side of a great table-land. That table-land is covered with snow. It is a great snow-field, and wherever a valley comes up into it this valley receives a river or tongue of ice, which descends from the snow-field and marches slowly away out to sea. Several of these glaciers are depicted in that diagram. Now and again it happens, as shown exactly in the centre of the diagram, that the table-land ends off at its edge not in a slope but in a cliff. In such a case the snow and ice pressed on from behind come to the edge of it as a great white crystalline wall, break off there, and tumble in huge fragments to the bottom. In some cases the *debris* of these ice-falls form, in the corry or valley below, a new kind of glacier. An illustration is furnished in this diagram on the right hand, representing the only glacier, I believe, on the mainland of Europe which actually comes down to the sea. It is made up of the ruins or waste of the ice broken off from the edges of the snow-field above, and consolidated below into a second kind of glacier, which comes sliding down, polishing and grooving the rocks on its way until it reaches the sea, and breaks off there into slices or "bergs." Sometimes these bergs slip off with so much force as to raise waves of considerable size along that silent fiord, and which inundate the huts of the Lapps built close upon the shore.

Now there must have been in our own country many scenes like those represented upon the wall. The whole west coast of Scotland was very much like the coast of Norway, only still more icy; or like the west coast of Greenland. Each of the long sea-lochs indenting our western coast was filled with solid ice. Great sheets of ice spread themselves far and wide over the mountains and low grounds of the interior, even into the very centre of England. Ireland, in the same way, was covered with a great pall of ice.

Now this ice was not stationary, like a sheet of snow such as we see even now in winter spread over the surface of the ground. Being continually augmented by fresh snowfalls, it was constantly moving downwards from the high grounds to the sea; and in the course of this seaward motion it produced its characteristic form of sculpture on the rocks—viz., the polishing, grooving, and striation which we have already traced. The greater part of the country was buried under one vast glacier, which, as it moved outwards and downwards, ground away the rocks, breaking off fragments from cliff, crag, and valley, and using these and all other detritus it could reach as a vast file or a mass of polishing powder to grind down the mountains and plains, and to produce

the vast mass of clay and stones still found covering so large a part of the country.

So much, then, for the solution of the first part of our problem—the agent by which the scratching and polishing of the rocks of Britain was effected.

Now let us turn to the materials which resulted from this scratching and polishing. I have said that the ice gave rise to a great deal of rough clay with stones, representing some of the substance worn by it from the general surface of the country. Look at any ordinary section of this stony clay and you observe that it differs in colour in different places; that in districts where the rocks are red the clay is apt to be red; that in others, where dark-coloured shales and black rocks abound, the clay has a dark gray or lead colour; and that in regions where yellow rocks are common, the clay has a fawn or yellow colour. This local character is not confined to the colour, but may be traced even among the stones of the clay. These have been derived chiefly from the rocks of the district in which they lie; the further the stones have travelled, the fewer as a rule are they in number and the smaller in size. But this would not have been the case if the whole of these materials had been carried by some great wave from a long distance. These local characteristics show us very clearly that the clay must have been elaborated somehow or other on the spot, or at least not far from the spot where we now find it. So abundant and conspicuous are the stones, that the clay containing them has been called boulder-clay. Now a little examination will show you that these stones, whenever they consist of a hard enough material, are polished and scratched in the same way as the solid rocks of the country. You could gather, I have no doubt, abundance of such scratched stones from the boulder-clays in the neighbourhood of Manchester. Take up one of them, and note how the marks have been graven on it. You see no indications of haste and tumult. The scratches are not put down at random, but lie in a general way parallel with each other, and with the long axis of the stone; that is, they run along the stone rather than across it. In this characteristic scratching of the stones is another argument in favour of land-ice, for underneath a vast heavy mass of ice, which was grinding and pushing them forwards, the stones would range themselves end-on, so to speak, that is, in the line of least resistance, so that they would be scored and scratched in that direction rather than in any other. The scratches sometimes cross each other, which indicates to you that the stones sometimes shifted their places a little under the ice.

A further examination of the stones in the boulder-clay will show you that most of them are local; only now and then do you meet with a fragment which has come from a great distance. By noting the probable localities from which the stones were transported you may even ascertain the direction in which the clay was moved, or, in other words, the quarter whence the great ice-sheet travelled which pushed the clay along with it. You see that the most abundant stones are those which have been derived from the rocks of the neighbourhood; that next in number come those from hills a few miles away; while from hills thirty or forty miles distant, or more, only two or three it may be out of every hundred are to be obtained.

These boulder-clays are sometimes considerably more than 100 feet thick. They are not always entirely masses of stony clay, but sometimes contain intercalated beds of sand, gravel, and fine clay. Let us look at some of these intercalations, and see what part of the story they have to tell us. In some districts, as, for instance, the centre of Lanarkshire, some of these intercalated beds consist of sand and clays, with thin peaty layers; and now and then with the remains of the mammoth, branches or trunks of trees. Since these interposed deposits rest upon and are covered by masses of ice-ground material, from which they themselves differ so greatly, they evidently represent an interval between two periods of cold. It is clear that in order to allow of the growth of trees, whose trunks are found in these clays, and to let the mammoth roam over the country and find sufficient vegetation there for his support, the ice must have retreated for a time, perhaps for a very long time, but at least long enough to let the lower parts of the country emerge from their arctic covering, and remain free of ice, and acquire a coating of vegetation. From these intercalated beds in beds in the boulder-clay, therefore, we learn that the great ice age was not one unbroken period of cold, but that it was broken up by one or more warm periods, which were perhaps of sufficient duration to permit many successive generations of forests and of animals to live upon the surface of the land.

We have now got up from the pavement of ice-worn rock to the top of the boulder clay. In the upper parts of this deposit, and even through its whole mass where it lies on the lower grounds far away from the higher hills, we meet with traces of the presence of the sea. These traces are furnished by shells and other marine organisms. Look for a moment at these shells. Compare them with shells living in our seas now, and you find a good many of

them to be not the same. Some of these shells belong, therefore, either to extinct species or to species which are not now found, or at least are exceedingly rare in our seas. But look at a collection of shells brought from the seas which wash the north coast of Norway, or still further north, and you will find there all the missing shells, of which you could find no living parallels in our own seas. They are what are called *arctic shells*—that is, have their congenial home in the arctic ocean. You see at once what an interesting corroboration they furnish of the evidence to be adduced from other quarters of the icy character of this great geological period.

The fact that marine shells occur in the upper clays indicates that the land must have been sinking below the sea; and the height to which we can trace these shells is of course the lowest limit to which the old sea-line reached. The land must have been at all events as deeply sunk in the sea as the height to which we can trace these shells. They have been found in this country at a height of at least 1,400 feet; but the deposits in which they lie range probably to at least 2,000 feet. Hence at the part of the ice age represented by these shelly clays, the land cased in ice had sunk below the sea, so as to be reduced to a mere archipelago of islands. The cold, moreover, still continued, because the sea, as we can show, must have frozen round the margin of these islands, just as it freezes round the margin of the land in Greenland now, forming what is there known as the *ice-foot*. This freezing of the sea is indicated by the occurrence of big blocks of stone lying in and upon those upper marine deposits which I have described. It was then that the "foundlings" were scattered far and wide over the country. Some of these blocks probably fell at first upon the surface of glaciers, and were thus carried down to the sea, where, as the ice broke off into icebergs, they suffered another and more prolonged transport; others loosened by frosts tumbled down on the sheet of ice formed along the margin of the land, and on the breaking up of this ice were likewise borne across the sea. Thus large blocks of granite from the south of Scotland and from Cumberland were scattered far and wide over all the submerged northern counties of England, and pieces of the highland rocks were borne across the broad central valley of Scotland, and left high upon the sides of the southern hills. It was at this time, too, that those strange mounds and ridges of sand and gravel were heaped up to which I referred at the beginning of this lecture.

Now after the extreme submergence of the country, we have proofs that there came a gradual re-elevation, and that, whilst the

country was rising, the cold still continued great. Evidence of this is furnished by relics of what are called valley glaciers; that is, tongues of ice reaching from snow-fields down into valleys, as distinguished from the great universal ice-sheet of earlier times. Suppose that we could at this moment transport ourselves to such a valley as that depicted in the diagram on the wall—to one of the wilder valleys in Wales for instance, such as the pass of Llanberis, or one of those which lead up into the lakes of Cumberland, or, wilder still, to one of those which strike into the heart of the Grampians—we should find, first of all, that the older clays, gravels, and sands, both those of the great ice-sheet and those of the marine submergence, have almost all been cleared out of the valley; that the rocks, along the sides and bottom, wherever they can be seen, are all polished and scored in the same way as those of the valleys and fiords of Norway. But one of the most singular features to arrest our attention would be the occurrence of great mounds and ridges of earth and stones running along the sides of the valley, or crossing it in great barriers. Such piles of rubbish, often with huge blocks of stone tumbled upon their surface, might be traced up to the very base of the mountain-cliffs. Not infrequently we should notice that they occur as successive barriers across the valley, with flat spaces between them, and that now and then behind the last barrier the drainage of the valley has been arrested, so as to form a little lake. Now each of these rough, boulder-strewn ramparts, called *moraines*, represents to us a stage in the retreat of the ice. Let me make this clear by means of one of the diagrams on the wall. This drawing represents the form of the ground at one locality in the south of Scotland where two glaciers once existed. I have taken the liberty of putting a snow-field on the top of the ridges, and restoring the two glaciers which formerly occupied the bottoms of the valleys. But such a liberty is fully justified by the evidence, as you will readily see. In the foreground there are first of all, on the right hand, great sheets of rock, well ice-worn, smoothed, and polished, having loose blocks of rock resting upon them. In the centre rough dark mounds of earth, forming the moraine, rise, as you notice, one over another, away up towards the left into a recess of the mountains, which descend abruptly in great cliffs. Behind the moraine mound, in the centre of the drawing, part of a small lake is seen. So far, therefore, the diagram only represents what you would actually see if you visited the place which it represents.

Now, how was it that these great mounds of rubbish came to choke up the valleys as they do? You can trace them up to the

base of the cliffs, and if you look at the large stones lying upon the mounds you can recognise them as parts of the same rocks as those of which the cliffs are composed. They have been moved from the places where they originally tumbled from the cliff, and have travelled, some a greater and some a less distance. Now, it was on the surface of the ice of the old glaciers that they performed this journey.

Imagine the hills to be once more covered with snow, and that, from the accumulated weight and pressure of the snow-fields, a glacier or tongue of ice is squeezed down each of these two valleys, as indicated in the diagram. The frosts of winter from time to time loosen great blocks of stone and quantities of earth and sand from the sides of the cliffs on either hand, and this rubbish, falling upon the surface of the glacier, is carried on the ice down to the point where the glacier melts. There the earth and stones are tumbled down somewhat as rubbish is shot to make a railway embankment. Sometimes the quantity of water escaping from the melting glacier is enough to sweep all this loose material away and spread it over the valley bottom. In other cases, the rubbish accumulates into those irregular mounds. When the glacier, owing to milder seasons, shrinks back up the valley, it leaves its former rubbish heaps and begins to make a new set at the place where it now melts away. Each successive retreat and pause of the ice may then be marked by alternate barriers of rubbish and flat level spaces. Now and then one of these barriers is so thrown down as actually to pond back the water escaping from the glacier, and thus to form a lake. This lake still exists in the case of the glacier to the right hand in the diagram. You see a little bit of it just appearing beyond the furthest of the moraine mounds. The glacier to the left hand also at one time had its lake but the latter has been drained by the gradual cutting away of the moraine barrier which kept it back—a fate which must, sooner or later, befall the remaining lake when it at last succeeds in cutting away the hindmost mounds of that great mass of rubbish which the glacier there threw across the valley.

Now what has taken place in the uplands represented in our diagram has taken place over and over again in all the higher mountainous tracts of this country. When the land began to rise again so great was still the cold that each main group of mountains and higher hills gave birth to an independent system of glaciers. From all our great valleys glaciers came out upon the low grounds. You may gain some notion of the intensity of the cold, even at this comparatively late part of the ice-age, when I tell you

that not only did this happen in the mainland, where there was a considerable mass of high ground—as, for instance, in the Grampians and in the uplands of Cumberland and Wales—but even detached islands had their glaciers. The island of Arran, for example, so limited in its area, had yet a snow-field large enough to send a glacier down each of the main valleys that led up into it. Nay, so great was still the severity of the climate that not a few of the glaciers of that time were sufficiently long and came down low enough actually to reach the sea, and to discharge their rubbish of earth and stones partly in salt water.

At this time Britain was probably still joined to the mainland, at the south-eastern end; so that rivers now flowing into the North Sea—the Thames, Humber, and others—appear then to have been tributaries of the Rhine. In looking at the present volume of water in our rivers, and comparing it with the traces to be found of the height to which they once reached, we are led to suspect that the rivers must once have been considerably larger than now. We may be sure, too, that when all this mass of snow and ice was melting, there could not but be a great deal of water in many of our rivers. Probably to this cause is owing the large quantity of gravelly detritus to be traced in not a few of our river valleys, and also the great height to which old river terraces can be followed. It would appear, too, that these larger rivers were frozen over in winter.

It is about this time of swollen rivers and severe winters that we begin first to encounter traces of man as an inhabitant of Britain. During the most intense parts of the ice age little evidence is to be met with as to what the plants and animals of the country were. Those intercalated beds of sand and gravel to which I have referred show us, indeed, a faint glimpse of some of the animals, such as the mammoth and the reindeer, which roamed through the old forests. These and other animals, whose bones are found in old river deposits and in caves, continued to haunt the country when the retreat of the ice and the subsequent spread of vegetation permitted.

When the rivers were in this condition, and probably when there were still glaciers in the country, the earliest men appear to have found their way across from the continent on the still unsevered land into Britain. We fix this as the probable time of man's advent, because it is in some of the river gravels of this period that we find the earliest human implements—those worked flints, which are now universally recognised as having been fashioned by the hands of the early human races.

The climate went on gradually getting milder and milder, and the northern animals were by slow degrees extirpated. The reindeer, the mammoth, and the musk sheep travelled north again, as the animals and plants of a more temperate climate appeared, until the whole fauna and flora, that is, the assemblage of animals and plants of the country, came to wear very much the aspect which they have now.

Strange it is to find in our land, even yet, traces of this struggle between the forms of the old ice age and the newer and more southern forms. If you dredge out in the deeper and colder parts of any of the seas around our islands, you will find living there some of the forms which were living abundantly in much shallower water during the ice age. They cannot compete in the warmer upper waters with the hosts of invaders from milder seas, and they have survived in those deeper abysses where they enjoy conditions nearer to those which they had in any part of the upper waters during the ice age. In these depths they seem to be lingering and dying out.

On the other hand, we find a corresponding change taking place upon the land. You are probably aware that if you climb any high mountain in this country you meet as you ascend with plants differing more and more from those to be found on the plains. These high-growing, or what are called Alpine plants, give a distinctive character to the vegetation of our higher mountains, forming an assemblage of plants which delight in bleakness and cold. Now these plants appear to be really descendants of the plants which once covered all or greater part of this country, during at least the later stages of the ice age. They are, in fact, arctic plants; and just as shells have been driven step by step from the shore waters down into the colder depths, so the plants have been driven away foot by foot from the lower plains up to the colder hill tops. There they are maintaining a struggle for existence. If the climate of this country should gradually get colder again, these arctic plants, rejoicing in the change, will by and by descend and reoccupy the plains from which they have been driven. If, on the other hand, the climate should get milder, then the more southern forms will by and by creep further and further up the hills until they finally drive the last of the arctic forms out of existence.

Such are some of the annals of that strange chapter of our country's history to which we give the name of "The Ice Age." We have been tracing, this evening, the evidence to be gleaned in Britain, but you may be well assured that phenomena marking so

strange and eventful a period in the history of this country could hardly have taken place without involving neighbouring lands. And in fact, we find that not in Britain only, but all over the northern half of Europe and of North America, evidence of exactly the same kind is to be obtained, proving that the cold which sealed up our own hills in ice was not a mere local feature, but extended over the northern hemisphere.

It would lead me far beyond the limits to which this lecture must be confined to enter on an explanation of the causes to which this wide-spread cold must be attributed. Let me only say that geologists are now beginning to understand that these causes are to be sought, not so much in any mere changes upon the earth's surface, as in our planet's relations to the sun; and that they are, therefore, of a recurrent kind, reappearing at widely-separated intervals, when the earth stands again in the same position towards the centre of our system.

In that marvellous history of the earth, of which only the mutilated fragments have been preserved to us in the rocks under our feet, traces exist of still older ice ages, faint, indeed, as we might very well expect they should be, but nevertheless of recognisable kinship with the last one. It is in searching out such fragments of the records of the past, and in linking them with the present, that geology, as it seems to me, makes some of her worthiest tributes to the dignity of science. What at first sight, for example, could seem more hap-hazard and abnormal than the descent of polar ice into the very heart of fertile Europe? And yet though we do not now know, and may never fully master the inner meaning of these recurrent ice ages, at least in their relation to the general life of the planet, we can indulge in the gratification arising from the conviction that we have at all events been enabled to see so far beneath the outer veil of seeming disorder as to realise that they are most certainly parts of the orderly system of nature; that as they have come round at vast intervals in the past, so will they return in the future as notes of that majestic rhythm to which the whole creation has been tuned.

THE SUN AND THE EARTH.

A LECTURE

BY PROFESSOR BALFOUR STEWART, F.R.S.

Delivered in the Hulme Town Hall, Manchester, November 13th, 1872.

I WILL first of all ask you to accompany me into a place with which we are all more or less familiar—I mean the room in which an engine does its work. Perhaps we are struck with the vast display of power before us, and with the noise and tumult that are the accompaniments of such power; for, in truth, an engine is a great worker, and we are none of us slow to take advantage of the fruits of its industry. The engine will probably be a steam-engine; it may possibly be an air engine; at any rate it will be a heat engine. Now, what are the conditions under which such an engine works? In all engines two things are necessary. We must first of all have a hot chamber; then we must have a cold chamber, and the engine will produce work in the process of carrying heat from the hot chamber to the cold. These are the essentials. The cranks, and wheels, and cylinders, and pistons are conveniences, but they are not really essential; we can conceive of an engine without them. Let us take an ordinary low-pressure engine. Here we have a boiler and a condenser, and as the engine works the heat is carried from the boiler to the condenser. Now, in order to show you that this is the case, and that the heat must be carried from a hot substance to a cold one, let us consider an engine of quite

a different kind,—I mean an ordinary fire or flue, for an ordinary fire is to some extent an engine. You have in it the production of mechanical effect in the process of carrying heat from a hot substance to a cold. In an ordinary fire we have the hot air rushing up the chimney from the fire and mingling with the cold air above—that is, we have the carrying of heat from a hot substance to a cold; the hot current of air rushing up the chimney and mingling with the cold air outside. And an engine of this kind—for it is an engine—really produces work, and you might take advantage of the current of air that is going up the chimney; indeed, an ordinary fire is a ventilating engine, and will do very often the work of ventilation that would otherwise require an ordinary engine.

Now let us consider two great heat engines; one I may call the Earth engine and the other the Sun engine. Let us consider first of all our own earth as a heat engine. Here is a model of the earth. [Exhibiting a globe.] You all know that the earth is a body which revolves round its axis in twenty-four hours, and that the equator, or the central part between the two poles of the earth, is that particular region on which the sun shines very directly; whereas, on the other hand, the poles are the regions where the sun hardly shines at all. The equatorial regions are the boilers of this heat engine, and the pole is its condenser; and this engine—for it is an engine—works in the process of carrying hot air from the equator to the cold parts. What takes place? The sun shines directly upon the equator, and this heated air, just as in the case of an ordinary fire, ascends to the top of the atmosphere, and is replaced by cold air blowing in from the poles. We have a cold current of air that blows in from the poles to the equator along the ground; and on the other hand the heated air finds its way in the upper regions of the atmosphere from the equator to the poles. If this heat engine were at rest, if it did not revolve, we should have a cold north wind blowing along the ground from the pole, and a warm south wind blowing from the equator towards the pole; but, inasmuch as it revolves on its axis in twenty-four hours, instead of a north wind blowing along the surface, you have a north-east wind, called the "trade wind;" and, instead of a south wind, you have a south-west wind, called the "anti-trade wind." Here, then, you have a series of winds blowing, by means of which heat is really carried from the hot parts to the cold parts of the earth.

There is still another way in which the earth acts as a heat

engine, and that is this: We have high above the earth a stratum of air extremely cold, and heat is constantly being carried from the warm moist air next the ground to this cold air next the cold clear sky. We have *ascending* currents carrying up this warm moist air into the upper regions: and very often this process cools the air to such an extent that it can no longer retain all the aqueous vapour and this aqueous vapour is deposited in the shape of rain, hail, or snow, and this ultimately finds its way to the ground; so that just as in an ordinary engine you have the cold water pumped back from the condenser to the boiler, for here the ground acts as the boiler and the upper regions as the condenser. Thus you have an arrangement by which heat is carried from the warm moist air next the earth to the cold air next the cold clear sky; and in this respect also the earth acts as a heat engine. Now the earth being a heat engine does work, and the seaman who hoists a sail, or the miller who grinds his corn by means of a windmill, really take advantage of the work done by the earth engine, as much as we do when we have work done by a low-pressure engine or a locomotive.

But again in the ordinary heat engine there are two things that keep the engine working smoothly. First of all we have the "governor" that regulates the supply of steam; and then we have the "fly wheel" that equalises the rate of motion of the engine; and by means of these two things the engine is kept working smoothly. But in the earth engine these arrangements appear at first sight to be entirely wanting; it seems to be an engine that works by fits and starts. For a long time we have a period of profound repose; then all at once you see that a storm is brewing, and especially in the regions next the equator these storms become terrific tempests, doing great damage, throwing down houses, tearing up trees, and devastating whole districts; while at sea they produce numerous shipwrecks. In such cases what is the matter with this earth engine? Is it not properly regulated? Is it gone mad? If so, let us endeavour to see if, at any rate, there be not some method in its madness.

In order to do this, we shall mount, if you please, into the celestial regions, and consider a still greater heat engine than even our own earth—I mean our sun—for the sun is also a heat engine. The sun is so far away that if a railway train at full speed were to go from the earth to the sun, it would take between 200 and 300 years to reach the sun; and it is so large that the same train would take about nine years to go round the surface of the sun. So you may imagine how large the sun is. If we were

to represent the sun on the same scale that we have here represented the earth, the globe would have a diameter of about 100 feet; in fact we could not get it into this room. We have therefore been obliged to reduce our scale. Here we have a model of the sun. The sun, like the earth, revolves upon its axis, but only once in 25 or 26 days. Well, I said that the sun was a heat engine. But in the first place I do not think there is any clear evidence of a system of polar and equatorial currents in the sun similar to those on the earth.

But although with regard to the sun we have no direct evidence of a series of currents of wind going from the sun's poles to the equator and back again, yet, on the other hand, we have very strong evidence of a series of ascending and descending currents going up and down in the sun's atmosphere, similar, no doubt, to those ascending and descending currents in the atmosphere of the earth. For, first of all, the temperature or heat of the sun is very much greater than the heat of the earth. You have in the sun a very hot substance, and a cold clear sky surrounding the sun as we have surrounding the earth. In the next place, let us think for a moment what it is that makes heated air go up. It is because this heated air becomes lighter; that is to say, it has less weight. Now, what does weight mean? It means the attraction of the earth. Without the attraction of the earth you would have no weight; and if you had an earth twice as heavy as this you would have double the attraction. Hence, at the surface of a large body like the sun the attraction is enormously greater than it is at the surface of the earth; and, consequently, if a thing gets lighter on the sun, it rises much more rapidly than the same thing would do on the surface of the earth, because the attraction is so very much greater. If there were no attraction, there would be no up-and-down motion at all; but when the attraction is very great, then the up-and-down currents are extremely strong. On these two accounts, therefore—both on account of the great difference of temperature between the surface of the sun and the cold sky about it, and on account of the great weight of the sun—these ascending and descending currents on the sun's surface are extremely strong. I should surprise you if I were to mention how strong they are. They move at the rate of something like 30 or 40 miles in a second at least. That is very much stronger than the strongest hurricane on the earth's surface. Well, as these ascending currents go up in the sun just like the currents on the earth, they carry with them a quantity of

vapour; I do not mean to say aqueous vapour, but very likely the vapour of heated metals, such as iron and magnesium. As these vapours ascend, they get into a colder region, and the vapours are deposited in the shape of clouds, just like the clouds of the earth's atmosphere, and they find their way down again to the sun's surface. Now remember that the sun shines by its own light, and consequently these cool vapours that are falling down in the shape of celestial rain or hail will appear darker than the bright parts of the sun that are not cool. If this be true, we ought to expect that when the sun is seen by a powerful telescope it should present a mottled appearance, consisting of black patches on a bright ground. Now I will show you by means of a magic lantern a picture of the sun, not taken from life, but a sketch of the sun as seen through a powerful telescope; and you will see that over all the surface of the sun you have this mottled appearance. You thus see that precisely the same thing is going on in the sun as in the earth, in the way of ascending and descending currents, only very much more powerfully. The ascending currents convey metallic vapour, and these are deposited when condensed by the cold, and find their way down as clouds, which appear as black patches when you examine the sun very closely. There is one thing I ought to mention. A storm of hail or rain on the earth is stopped by the surface of the earth; but the sun is so hot there is no solid or liquid surface. Very likely the sun is one mass of gas, and the surface that we see, instead of being solid or liquid substance, is one of cloud.

Now here you see a number of bright patches and a number of black patches. No doubt these bright patches are the hotter parts of the sun, and these black patches denote something like solar rain, or matter that has been cooled by contact with the upper regions of the solar atmosphere. Now let us imagine ourselves traversing the cloudy surface of the sun, and what shall we see? Perhaps, as we travel over the solar surface, we may come to the brink of a most enormous chasm; a chasm so enormous that, as far as surface extent is concerned, it might swallow up twenty or thirty worlds like our earth, not, perhaps, so deep, but, at any rate, something like 3000 or 4000 miles deep. This chasm would have sloping sides that are somewhat less luminous than the sun's surface, and it would have a bottom still darker than the sloping sides; in fact, you would have three gradations of light—the bright surface of the sun, the sloping sides of the cavern (less bright), and you would have the black bottom of the cavern that

was still less bright. The sloping sides are called the "penumbra," or half-shadow, and the bottom the "umbra," or whole shadow. This is, in fact, what is called a "sun spot." And now I should like to show you in the magic lantern one of these sun spots, not taken from life, but from a picture of the sun as sketched by an observer with his telescope. Here you see a picture of these sun spots. I think these are sun spots in the act of breaking up. Here is another picture of sun spots, and at the right you will see a number of small bright patches, and one of these bright patches is in the act of crossing over a spot.

Now you will ask me how I know that these sun spots are caverns at all, because in the telescope they give us no appearance of caverns; how, then, do we know that they are caverns? Well, if a sun spot were a cavern, what kind of appearance would it have? Here is the model of the sun with a hole in it to represent a cavern. I will make the cavern turn round just as a sun spot would, and you will see what happens. As the cavern turns into view at the edge you see the sloping side farthest away from the apparent centre of the sun, but the sloping side next the centre is entirely hidden from your view. When it comes to the centre you see both sides equally; then as it turns round by the rotation of the sun—for the sun revolves on its axis—the same thing happens on the other side. If, therefore, you see the spot at the edge of the sun, and if the spot is a cavern of this kind, the appearance presented should be a black patch, and a sloping side further from the centre than the black patch, the other sloping side being hidden from view.

Now I will show you a photograph of the sun, taken from life at Kew Observatory, and those who are near the picture will see that the sloping side is farther away from the centre than the black spot. But bear in mind that this is a negative, and consequently the bottom of the spot will appear very bright, instead of very dark. Those who are near will see that there are two bright patches showing the dark bottom of the sun spot, and a sort of half-bright patch away from the centre. That shows that the penumbra, or sloping side next the centre, has to a great extent been hidden from our view; in fact, it shows that the sun spot is a cavern.

Now I will give you one more proof that the sun spot is a cavern. In that picture you saw some bright patches sailing over the sun spot, just as a bird might fly across the mouth of a pit. That shows that the spot is a pit, because, if it were a cloud,

those patches would be hidden from view. So much for the black patches, or sun spots. I should like now to say a few words about the bright patches, or faculæ of the sun, because the sun has bright as well as black patches. I do not know that we can make this visible through the whole length of the room, but at any rate we will do our best. I shall show you once more this negative picture of the sun taken at the Kew Observatory, and you will see on it an extremely black patch, or a cloud as it were. But bear in mind that the picture is a negative, and that this black patch really denotes a very bright one, because you know that in a negative things go by contraries. I will also ask you to note another thing in this negative. You will see that the centre of the sun is very much blacker than the rim; which means, in reality, that the visual centre of the sun is brighter than the rim, because this is a negative picture.

I have thus endeavoured to show that on the sun's surface we have not only black patches, but we have also bright patches. Now what are these bright patches? The black patches I showed you before were caverns in the sun's atmosphere. I do not mean to say that they were caverns without anything to fill them, because the sun's atmosphere would fill the caverns. What are these bright patches? Just as the sun's spots were caverns, we have great reason for supposing that these bright patches denote matter, hot and bright, which has been thrown up in the atmosphere of the sun. And I will give you one proof of this. I showed you in this negative picture of the sun that it appeared to be blackest in the centre, and not quite so black round the rim, and I told you that it meant that the sun was brightest in the centre. • Now why is that? The sun is not quite so bright round the rim, because the bright part of the sun is enveloped in an atmosphere that is not quite so hot as itself. There is a comparatively cold atmosphere surrounding the sun, and you see the rim of the sun slantingly through a great depth of this cold atmosphere; consequently, the rays that come from the rim of the sun have to pierce through a great depth of this comparatively cold atmosphere. Well, supposing however that any matter is thrown up to the top of the sun's atmosphere, it will make a great difference near the rim, whether you see this bright matter at the bottom or top of the atmosphere. That is to say, at the rim of the sun this bright matter will gain a great deal in light by being thrown up to the top of this absorbing atmosphere; hence faculæ are seen most distinctly at the rim of the sun. At the centre of

the sun we cannot distinguish the faculæ. This proves that the faculæ are bright matter or currents thrown up into the air of the sun; and this accounts for the changes of luminosity.

Now any object can be seen in two ways. You can see a thing in a "ground" plan, or you can see it in an "elevation." A tree for instance can be seen in two ways; it may be seen as a bird would see it, or as from a balloon; or you may see it in the more picturesque way in which you generally see it, that is to say, with the sky on the other side of it. The first is the ground plan, and the second is the elevation. So there are two ways of showing these bright patches or ascending currents, that are thrown up into the atmosphere of the sun. We may see them as projected upon the sun's surface; or we may see them as extended beyond the sun's surface and projected on the sky. What I showed you in the picture was a bright ascending current seen in the ground plan, as projected upon the surface of the sun. Well, the question is, how can we see the current or its upper part extending beyond the sun's surface, and projected upon the sky? Ordinarily we cannot see it, because if you were to examine the rim of the sun through a telescope, no doubt beyond the rim of the sun there might be these currents; but the general glare of the sun is so great, that is to say the reflection of the sun's light from the atmosphere is so great, that it would entirely cloak the thing from our view. There are two methods, however, by which we can overcome this difficulty, and see these bright streams in the sun in elevation as it were, or projected against the sky beyond. And one of these methods is to observe them during the few precious moments of a total eclipse. When the moon comes between the sun and the earth it cuts off the light of the sun itself, and consequently cuts off the atmospheric glare, so that the eye is not dazzled by it, but is able to perceive the true features of the matter that is just beyond the rim of the sun. Now I will show you a picture of the sun which was taken by Mr. Brothers, the photographer, of Manchester, during the last total eclipse at Sicily. Here we have a picture of a total eclipse of the sun, and you see surrounding the disc of the sun some reddish matter first of all, and beyond that we have a great quantity of bright matter. Now this reddish matter is really these bright currents that I spoke about, seen in elevation or projection beyond the sun. They consist of large masses of hydrogen, which seems to play the same part in the solar atmosphere that oxygen and nitrogen play in ours, and besides that, they contain also the vapours, sometimes of iron, sometimes of magnesium,

sometimes of sodium, that are carried up by the ascending currents into the sun's atmosphere. This picture of the total eclipse of the sun is taken from life, as it were; the only thing added to it is a little red colour. From this you see what terrible currents and changes must be taking place all round the surface of the sun. Now I will show you a picture of these up-bearing heated currents seen in elevation, or against the sky; but it is taken by another method, which I shall presently describe. Here you see enormous currents taking place, and sometimes assuming the appearance of a fire, sometimes of a cloud, and sometimes appearing like gigantic fiery trees, of which the tops would be something like a hundred thousand miles above the surface of the sun, and of which the branches grow, as it were, at the rate of thirty or forty miles a second. I will show you another picture of the same thing. Here you have an uprushing in a spiral shape, with a sort of cloud at the top of it. You see what strange appearances may be seen surrounding the rim of the sun. Now there is very little doubt that these are the upper parts of the same appearances of which faculæ form the lower parts. The faculæ are seen projected against the surface of the sun, but these red flames are seen in elevation against the sky beyond. They are both connected together.

A tree, you all know, would appear quite a different thing seen by a person looking at it from above, and projected on the earth, to what it would when seen in the ordinary way. It would be much less picturesque when seen from a balloon or by a bird, than it is when projected against the sky. And so these currents are much less picturesque when seen as faculæ, that is to say, when seen projected upon the surface of the sun, than when extended beyond the rim of the sun on to the clear sky beyond.

Now there are some questions that I should like to reply to. First of all you will naturally ask me, how do we know that these bright ascending currents on the sun's surface are composed of hydrogen and of the vapours of iron, magnesium, sodium, &c., carried up into the upper regions of the solar atmosphere? Well, in order to find out what these bright vapours are really composed of we resort to a contrivance which is called the spectroscope. The spectroscope has the means of analysing the light that reaches us, and telling us really what a burning object is, whether it be in the room here, or in the sun, or in a fixed star, or where-ever it be. Here we have the bright light of an electric lamp, and

we will pass the light through two prisms, which will bend the rays and throw them upon this screen. Now you see on the screen a beautifully-coloured band, and you see that the light which originates this band comes from the slit in the electric lamp. Now what is this band a sign of? It is a sign that this light at the present moment contains all the possible kinds of light; one kind is thrown on one part of the screen, and another kind on another part of the screen; and altogether we have every possible kind of light coming from the electric lamp and passing through this prism. Now I will place behind this slit—instead of the light from incandescent carbon, or charcoal, such as we have here—a metallic vapour; we will begin with the vapour of the metal thallium, a metal lately discovered. We will see whether the vapour from this metal really contains every kind of light, or whether it does not. You see that the light now does not consist of every kind of ray, but consists of certain bands—a bright ray and a greenish blue one. Here, then, we have a metallic vapour that does not give us every kind of light—it only gives us two little rays of light; and it will be quite the same whether I burn the thallium here, or whether the thallium was burning in the sun, and by means of a spectroscope, or in other ways, I brought the rays of light from the sun to this slit here. In either case you would see precisely these two bright rays of light; and you would know by the position of these two bright rays on the screen what the metal was that was giving out the light. You could tell it was the metal thallium by the position of the rays; and provided you keep everything stationary here, it is a matter of no consequence whether the thallium is burning here or in the sun.

Now I will show you one or two more metals. The next metal that I will show you is the metal lithium. And now, while these experiments are going on, I should like to say a word about how it is that by means of the spectroscope we can see the shapes of these red prominences beyond the sun's surface, while we cannot see them on ordinary occasions. I told you that on ordinary occasions if you look at the surface of the sun, the glare of the sun, reflected from the atmosphere of the earth, spoils everything, and you cannot see it. But this glare is composed of many different kinds of light, and when you make this glare pass through the prisms it is scattered all over the screen. On the other hand, the light that comes from these red flames and from these up-currents consists only of one or two definite rays of light, and they are not scattered. The consequence is, that while the prisms

scatter the rays of light that come from the atmospheric glare, they do not scatter the rays that come from the red flames. And so by means of the spectroscope you are enabled to see the red flames quite distinctly, because you have destroyed the atmospheric glare by scattering it. That is the secret of the success achieved by Mr. Janssen and Mr. Lockyer, and by means of which we can see these red flames whenever we choose.

I will now show you the spectrum of silver vapour, and you will see what lines it gives out. Here you see are several bright lines denoting the existence of silver, and the spectrum of silver will always show the bright lines seen on this part of the screen. The presence of these particular lines will invariably indicate the existence of burning silver vapour—not solid silver, or even liquid silver, but silver in a state of vapour.

And now last of all I will show you the spectrum of the metal sodium. You will see that instead of having several lines, there is only one intensely yellow line. You see a dark line, which I cannot explain at the present moment; but you will notice in a short time that that dark line will be replaced by something different. Now we have got a bright yellow line; in fact, the light which incandescent sodium gives out is intensely yellow—the same that is produced by putting salt into a flame.

I have thus endeavoured to show you that metallic vapours, unlike heated charcoal, give out only one or two kinds of light, and that by noticing where these lines of light are we can tell what are the substances that are burning, whether they be burning here or in the sun. And thus you see we can tell whether these up-rushing currents consist of hydrogen, or carry with them sodium, magnesium, and iron.

You will ask, how do we know that these currents are rushing up at the rate of thirty or forty miles in a second? Well, I cannot explain to you very fully during this lecture how we know this, but I may just tell you that if a metal or anything is burning and giving out light, and at the same time is moving very rapidly towards the eye, then the light that it gives out will be thrown upon one part of the screen; and if it is shooting very rapidly from the eye, the light it gives out will be thrown upon a slightly different part of the screen; the note, as it were, is changed. Just as a railway engine, in rapidly approaching a station, gives out one note, and in rapidly leaving it gives out another, so the kind of light given out by burning a body is slightly different according

as this body is shooting towards the eye or from the eye of the observer. By that means we can tell whether the incandescent sodium or hydrogen is shooting from or to the eye. We can find out that sometimes it is rushing upwards at the rate of thirty or forty miles a second, and sometimes rushing downwards to the sun's surface at the same rate.

Now, having given you the evidence for all this, you see at once that there must be enormous storms taking place on the surface of the sun—that this sun-engine, as it were, is always working very powerfully and very strongly; and no doubt in the case of the sun spot we see the cooled matter or cloud which is falling down; and as I told you that the sun, unlike the earth, is not a solid or liquid surface, but merely cloud, you can imagine that this shower goes down a long way before it gets into the hot regions and becomes melted. I have no doubt that is an explanation of the fact that the bottom of the spot is far below the surface of the sun. Well, these great disturbances on the sun's surface, or sun storms, take place in the equatorial regions, just as in the equatorial regions of the earth we have great cyclones and hurricanes. These sun storms which give rise to the sun spots occur near the equator, and do not take place at the poles at all. Here, then, is a curious likeness between the sun and the earth. And now comes, perhaps, the strangest part of the whole thing. If we notice the sun from year to year we find that some years there are very few of these spots or storms in the sun, and some years there are a great many. I have in a diagram here a record of the sun's spots that were observed during forty years, by a very accurate German astronomer, Hofrath Schwabe. You will see marked in red ink the various years in which we had the maximum of sun spots. In certain other years there were very few spots, such as 1834, 1843, and 1856. You know very well that the sun heats the earth, but besides that the sun sends us other rays that we do not see, and which are prolific in furthering vegetation and chemical changes in plants. We call these the actinic rays, and we are very much indebted to Dr. Roscoe for our knowledge of these rays, for he has told us all about them. I am indebted to him for this particular diagram, in which you see that at places near the equator, such as Cairo, we have a great quantity of such rays; at Naples not quite so many; at Manchester still less; while at Melville Island, a place very far north, we have hardly any of these rays.

I mentioned that in some years you have hardly any spots in

the sun, and in other years a great many; and the question arises: Have we most of the actinic rays when we have fewest spots; or have we fewest of these rays at the time when we have most spots on the sun? For the spots really appear to take away something from the light of the sun, and we might expect that they would take away something from these rays also. Now at the present moment we can hardly distinctly answer this question. Dr. Roscoe has invented an instrument which is going to be used by the Russian government; and in the course of ten years we shall be able more distinctly to tell whether this is the case. At present there is a strong suspicion that it is the case. There was a curious coincidence noticed by a student at Owens College, between what may be called the "good wine years" in Germany and the years of minimum sun spots. We have here a record of the years of fewest sun spots, and a record of the years in which the vine flourished best, and you see how well they coincide. This leads us to suspect that there is some connection between the effect of the sun upon vegetation and the spots. So much for the heat of the sun and so much also for the actinic rays of the sun. And now comes a very curious circumstance. It appears that the behaviour of these sun spots depends upon the position of the planets, and more particularly upon the position of the planets Mercury and Venus. Mr. Delarue, Mr. Loewy, and myself have measured every spot that was taken by Carrington, and every spot observed at Kew Observatory, in order to be accurate in the statement of our facts; and as far as we can tell by what we have done, the behaviour of the sun spots appears to be of this kind. Here is a model of the sun. Assume the planet Venus to be between you and the sun. The sun is turning round in this way, and as the sun's surface comes near to Venus there will be few spots; but as the surface goes away from Venus the spots will gradually increase, and attain their maximum at that particular part of the sun which is farthest away from Venus. And as that part of the sun's surface comes back again to Venus, the spots will get less: so that, in fact, the spots would be least at that part of the sun which was close to Venus, and greatest at that part farthest away. The same thing holds with regard to the planet Mercury. [Professor Stewart farther explained the increase and decrease of the spots with the aid of the model of the sun.] All these things taken together, and especially when Mercury shows the same kind of influence upon the spots as Venus, lead us to consider that it is extremely probable that there is some connection between the

positions of planets and the behaviour of the sun spots. Now, where we have action we have always reaction. If the planets influence the sun spots, no doubt the sun spots must influence the planets. And now let us see whether the spots on the sun influence the earth in any other way than those already mentioned. First of all, during those years when we have most sun spots we have most of what is called "magnetic disturbance." The earth is a gigantic magnet, and this magnet is very much disturbed on certain occasions, and consequently the needle vibrates and oscillates very quickly through small spaces. Further, a record has been kept at different observatories, and more especially at the Kew Observatory, of these disturbances of the earth's magnetism. Now it so happens that at those years when we have most sun spots we have most of these disturbances. Here, marked in red, you see the years of the most sun spots, and in this diagram you see the years when we have most magnetic disturbances. The correspondence is very marked in the years 1848 and 1859. Last year there were a great many sun spots, and last year there were also a great many magnetic disturbances. So that you see the earth is disturbed as a magnet at those times when most changes are going on in the sun.

Well, another curious fact is this: whenever the earth is greatly disturbed as a magnet you have great outbreaks of the aurora borealis. I told you that last year we had a great many sun spots and a great many disturbances of the magnet, and you will bear me out when I say that we had a great many appearances of the aurora borealis. I have here a diagram showing the number of occurrences of the aurora at Dunse, in Scotland. It does not pretend to extreme accuracy, because sometimes there would be a great deal of rain, and then the aurora may have escaped notice; but you see that there is a correspondence in the diagrams in the years 1837, 1847, and 1859. So that, approximately speaking, you see that the years of most auroras seen at Dunse are also the years of most sun spots, as given by Schwabe, the German observer, and also the years of most magnetic disturbances, as shown by the variations of the magnet at Kew and Toronto.

Well, thus we see that when we have most sun spots we have most magnetic disturbances, and most appearances of the aurora borealis. But besides this there is something more. My friend Mr. Meldrum, who is a great meteorologist and observer in the Mauritius, has been at the trouble of collecting all the logs of vessels that have crossed the Indian Ocean for the last twenty or

thirty years, and he can tell consequently of every cyclone or circular whirlwind that has taken place during that time in the Indian Ocean. Well, he finds that during the time of maximum sun spots you have most cyclones in the Indian Ocean. And he tells me lately that he has found that the same law holds with regard to the West Indian hurricanes.

Thus you see that the earth is very seriously affected by what takes place in the sun,—that both the earth and the sun engines work together; or you may say that they are companions in their irregularities; that whenever the sun engine is irregular, the earth engine appears to follow it and become irregular too. This investigation might strike you at first sight as being something very speculative and of no use, but you now see that it becomes of considerable practical importance. We are all implicated in it, and all very much affected by what takes place in the sun. I may also mention that Mr. Baxendell, the observer in Manchester, has found out that what we may call the convection currents of the earth, or general system of currents in the earth, is different at the time of maximum and the time of minimum sun spots. Well, here we have an extraordinary action and reaction taking place. And there is still one thing behind that I have not ventured to put into a diagram,—and that is the suspected connection between epidemics and the appearance of the sun's surface. One of the best known vegetable epidemics is that of the potato disease. The years 1846, 1860, and 1872 were bad years for the potato disease. Now those years are not very far from the years of maximum sun spots. This year or last year some of you may even have seen sun spots through the mists of Manchester with the naked eye. Here, then, we have at any rate a suspicion of some curious connection between these diseases affecting plants and the state of the sun. And, indeed, of late we have come to think that these diseases take place by means of germs; and consequently if anything is wrong with the atmosphere we may well suppose there will be some particular change of this kind, because these germs are carried by the atmosphere and live in the atmosphere.

There is still another curious point of interest in connection with the disease that took place about three centuries since, of a periodical and very violent character, called the "sweating sickness." That disease took place about the end of the fifteenth and the beginning of the sixteenth century. It took place in the following years—1485, 1506, 1517, 1528, and 1551, about a

period of eleven years intervening between the outbreaks. Now this is exactly the sun spot period. Can we tell what the state of the sun was during the outbreaks of this sweating sickness? We cannot with any certainty, because there were no observers of the sun spots in those days. But I told you that there was another phenomenon that accompanied sun spots, namely, when there were most spots in the sun you have most outbreaks of the aurora borealis. If there were unusual and frequent appearances of the aurora in those days they would probably be chronicled not as the aurora borealis, but as fires in the air and fighting armies. Turning to our old records, we find certain displays of the aurora mentioned, and they give a good idea of what must have been the years of maximum sun spots in those times, and curiously enough these are by no means far from the years of the outbreak of the sweating sickness. There is only a difference of about a year and a half on the whole, and the difference always seems to lie in the same direction. Consequently we are pretty certain that, at any rate, the outbreaks of this sweating sickness took place much nearer the time of maximum than the time of minimum sun spots. This, however, is still conjecture, and all these things require to be verified by further research. At any rate I think I have succeeded in convincing you that the problem is a very important one, and that a connection of some mysterious kind between the sun and the earth is more than suspected—it is extremely probable, though we do not know all about it. It is a problem that requires, before it can be properly worked out, not only great time and great trouble, but it requires great means, greater means than private observers can possibly afford; in fact it is a problem that ought to be taken up by all the civilised governments of the world, because unless it is taken up by these governments we shall never come to the bottom of the thing. Mr. Delarue, Mr. Carrington, Mr. Schwabe, Mr. Lockyer, Dr. Huggins, Dr. Roscoe, and myself have done all we possibly could, and we really cannot do anything more; and if the sun is to be observed regularly, year after year, and each spot marked, measured, and numbered, and all the other essentials of solar observation attended to, it is a work that private observers cannot possibly undertake, and it must be undertaken by the civilised governments of the world. Well, I wish we could incite our own British government to take the lead in this honourable career; but I am sorry to say that as far as science is concerned, I do not think we can. Other governments, such as those

of America and France, and many other governments on the continent of Europe, are willing to do much more for science than our government is. I trust, however, that when we have again occasion to push this matter upon the attention of our government, the people of Manchester, Liverpool, Leeds, Birmingham, and all the large towns in the north of England, will insist that the claims of this great problem shall no longer be disregarded.

ATOMS.

A LECTURE

BY PROFESSOR CLIFFORD, M.A.,

Delivered in the Hulme Town Hall, Manchester, Nov. 20th, 1872;

Also before the Sunday Lecture Society, in London, on the 7th of January, 1872.

IF I were to wet my finger and then rub it along the edge of this glass; I should no doubt persuade the glass to give out a certain musical note. So also if I were to sing to that glass the same note loud enough, I should get the glass to answer me back with a note.

I want you to remember that fact, because it is of capital importance for the arguments we shall have to consider to-night. The very same note which I can get the tumbler to give out by agitating it, by rubbing the edge, that same note I can also get the tumbler to answer back to me when I sing to it. Now, remembering that, please to conceive a rather complicated thing that I am now going to try to describe to you. The same property that belongs to the glass belongs also to a bell which is made out of metal. If that bell is agitated by being struck, or in any other way, it will give out the same sound that it will answer back if you sing that sound to it; but if you sing a different sound to it then it will not answer.

Now suppose that I have several of these metal bells which answer to quite different notes, and that they are all fastened to a set of elastic stalks which spring out of a certain centre to which they are fastened. All these bells, then, are not only fastened to these stalks, but they are held there in such a way that they can spin round upon the points to which they are fastened.

And then the centre to which these elastic stalks are fastened or suspended, you may imagine as able to move in all manner of directions, and that the whole structure made up of these bells and stalks and centre is able to spin round any axis

whatever. We must also suppose that there is surrounding this structure a certain framework. We will suppose the framework to be made of some elastic material, so that it is able to be pressed in to a certain extent. Suppose that framework is made of whalebone, if you like. Now this structure I am going for the present to call an "atom." I do not mean to say that atoms are made of a structure like that. I do not mean to say that there is anything in an atom which is in the shape of a bell; and I do not mean to say that there is anything analogous to an elastic stalk in it. But what I mean is this—that an atom is something that is capable of vibrating at certain definite rates; also that it is capable of other motions of its parts besides those vibrations at certain definite rates; and also that it is capable of spinning round about any axis. Now by the framework which I suppose to be put round that structure made out of bells and elastic stalks, I mean this—that supposing you had two such structures, then you cannot put them closer together than a certain distance, but they will begin to resist being put close together after you have put them as near as that, and they will push each other away if you attempt to put them closer. That is all I mean then. You must only suppose that that structure is described, and that set of ideas is put together, just for the sake of giving us some definite notion of a thing which has similar properties to that structure. But you must not suppose that there is any special part of an atom which has got a bell-like form, or any part like an elastic stalk made out of whalebone.

Now having got the idea of such a complicated structure, which is capable, as we said, of vibratory motion, and of other sorts of motion, I am going on to explain what is the belief of those people who have studied the subject about the composition of the air which fills this room. The air which fills this room is what is called a gas; but it is not a simple gas; it is a mixture of two different gases, oxygen and nitrogen. Now what is believed about this air is that it consists of quite distinct portions or little masses of air—that is, of little masses each of which is either oxygen or nitrogen; and that these little masses are perpetually flying about in all directions. The number of them in this room is so great that it strains the powers of our numerical system to count them. They are flying about in all directions and mostly in straight lines, except where they get quite near to one another, and then they rebound and fly off in other directions. Part of these little masses which compose the air are of one sort—they are called oxygen. All those little

masses which are called oxygen are alike; they are of the same weight; they have the same rates of vibration; and they go about on the average at a certain rate. The other part of these little masses is called nitrogen, and they have a different weight; but the weight of all the nitrogen masses is the same, as nearly as we can make out. They have again the same rates of vibration; but the rates of vibration that belong to them are different from the rates of vibration that belong to the oxygen masses; and the nitrogen masses go about on the average at a certain rate, but this rate is different from the average rate at which the oxygen masses go about. So then, taking up that structure which I endeavoured to describe to you at first, we should represent the state of the air in this room as being made up of such a lot of compound atoms of those structures of bells and stalks, with frameworks round them, that I described to you, being thrown about in all directions with great rapidity, and continually impinging against one another, each flying off in a different direction, so that they would go mostly in straight lines (you must suppose them for a moment not to fall down towards the earth), excepting where they come near enough for their two frameworks to be in contact, and then their frameworks throw them off in different directions: that is a conception of the state of things which actually takes place inside of gas.

Now, the conception which scientific men have of the state of things which takes place inside of a liquid is different from that. We should conceive it in this way: We should suppose that a number of these structures are put so close together that their frameworks are always in contact; and yet they are moving about and rolling among one another, so that no one of them keeps the same place for two instants together, and any one of them is travelling all over the whole space. Inside of this glass, where there is a liquid, all the small particles or molecules are running about among one another, and yet none of them goes for any appreciable portion of its path in a straight line, because there is no small distance that it goes without being in contact with others all around it; and the effect of this contact of the others all around it is that they press against it and force it out of a straight path. So that the path of a particle in a liquid is a sort of wavy path; it goes in and out in all directions, and a particle at one part of the liquid will, at a certain time, have traversed all the different parts one after another.

The conception of what happens inside of a solid body, say a crystal of salt, is different again from this. It is supposed that the very small particles which constitute that crystal of salt do not

travel about from one part of the crystal to another, but that each one of them remains pretty much in the same place. I say "pretty much," but not exactly, and the motion of it is like this: Suppose one of my structures, with its framework round it, to be fastened up by elastic strings, so that one string goes to the ceiling, and another to the floor, and another to each wall, so that it is fastened by all these strings. Then if these strings are stretched, and a particle is displaced in any way, it will just oscillate about its mean position, and will not go far away from it; and if forced away from that position it will come back again. That is the sort of motion that belongs to a particle in the inside of a solid body. A solid body, such as a crystal of salt, is made up, just as a liquid or a gas is made up, of innumerable small particles, but they are so attached to one another that each of them can only oscillate about its mean position. It is very probable that it is also able to spin about any axis in that position or near it; but it is not able to leave that position finally, and to go and take up another position in the crystal; it must stop in or near about the same position.

These, then, are the views which are held by scientific men at present about what actually goes on inside of a gaseous body, or a liquid body, or a solid body. In each case the body is supposed to be made up of a very large number of very small particles; but in one case these particles are very seldom in contact with one another, that is, very seldom within range of each other's action; in this case they are during the greater part of the time moving separately along straight lines. In the case of a liquid they are constantly within the range of each other's action, but they do not move along straight lines for any appreciable part of the time; they are always changing their position relatively to the other particles, and one of them gets about from one part of the liquid to another. In the case of a solid they are always also within the range of each other's action, and they are so much within that range that they are not able to change their relative positions; and each one of them is obliged to remain in very nearly the same position.

Now what I want to do this evening is to explain to you, so far as I can, the reasons which have led scientific men to adopt these views; and what I wish especially to impress upon you is this, that what is called the "atomic theory"—that is what I have just been explaining—is no longer in the position of a theory, but that such of the facts as I have just explained to you are really things which are definitely known and which are no longer

suppositions; that the arguments by which scientific men have been led to adopt these views are such as, to anybody who fairly considers them, justify that person in believing that the statements are true.

Now first of all I want to explain what the reasons are why we believe that the air consists of separate portions, and that these portions are repetitions of the same structures. That is to say that in the air we have two structures really, each of them a great number of times repeated. Take a simple illustration, which is a rather easier one to consider. Suppose we take a vessel which is filled with oxygen. I want to show what the reasons are which lead us to believe that that gas consists of a certain structure which is a great number of times repeated, and that between two examples of that structure which exist inside of the vessel there is a certain empty space which does not contain any oxygen. That oxygen gas contained in the vessel is made up of small particles which are not close together, and each of these particles has a certain structure, which structure also belongs to the rest of the particles. Now this argument is rather a difficult one, and I shall ask you therefore to follow it as closely as possible, because it is an extremely complicated argument to follow out the first time that it is presented to you.

I want to consider again the case of this finger glass. You must often have tried that experiment— that a glass will give out when it is agitated the same note which it will return when it is sung to. Well, now, suppose that I have got this room filled with a certain number of such atomic structures as I have endeavoured to describe--that is to say, of sets of bells, the bells answering to certain given notes. Each of these little structures is exactly alike, that is to say, it contains just the bells corresponding to the same notes. Well, now, suppose that you sing to a glass or to a bell, there are three things that may happen. First, you may sing a note which does not belong to the bell at all. In that case the bell will not answer; it will not be affected or agitated by your singing that note, but it will remain quite still. Next, if you sing a note that belongs to the bell, but if you sing it rather low, then the effect of that note will be to make the bell move a little, but the bell will not move so much as to give back the note in an audible form. Thirdly, if you sing the note which belongs to the bell loud enough, then you will so far agitate the bell that it will give back the note to you again. Now exactly that same property belongs to a stretched string, or the string of a piano. You know that if you sing a certain note in a room where there is a piano,

the string belonging to that note will answer you if you sing loud enough. The other strings won't answer at all. If you don't sing loud enough the string will be affected, but not enough to answer you. Now let us imagine a screen of piano strings, all of exactly the same length, of the same material, and stretched equally, and that this screen of strings is put across the room; that I am at one end and that you are at another, and that I proceed to sing notes straight up the scale. Now while I sing notes which are different from that note which belongs to the screen of strings, they will pass through the screen without being altered, because the agitation of the air which I produce will not affect the strings. But that note will be heard quite well at the other side of the screen. You must remember that when the air carries a sound it vibrates at a certain rate belonging to the sound. I make the air vibrate by singing a particular note, and if that rate of vibration corresponds to the strings the air will pass on part of its vibration to the strings, and so make the strings move. But if the rate of vibration is not the one that corresponds to the strings, then the air will not pass on any of its vibrations to the strings, and consequently the sound will be heard equally loud after it has passed through the strings. Having put the strings of the piano across the room, if I sing up the scale, when I come to the note which belongs to each of the strings my voice will suddenly appear to be deadened, because at the moment that the rate of vibration which I impress upon the air coincides with that belonging to the strings, part of it will be taken up in setting the strings in motion. As I pass the note, then, which belongs to the strings, that note will be deadened.

Instead of a screen of piano strings let us put in a series of sets of bells, three or four belonging to each set, so that each set of bells answers to three or four notes, and so that all the sets are exactly alike. Now suppose that these sets of bells are distributed all over the middle part of the room, and that I sing straight up the scale from one note to another until I come to the note that corresponds to one of the bells in these sets, then that note will appear to be deadened at the other end, because part of the vibration communicated to the air will be taken up in setting those bells in motion. When I come to another note which belongs to them, that note will also be deadened; so that a person listening at the other end of the room would observe that certain notes were deadened, or even had disappeared altogether. If, however, I sing loud enough, then I should set all these bells vibrating. What would be heard at the other end of the room? Why just the

chord compounded out of those sounds that belonged to the bells, because the bells having been set vibrating would give out the corresponding notes. So you see there are here three facts. When I sing a note which does not belong to the bells, my voice passes to the end of the room without diminution. When I sing a note that does belong to the bells, then if it is not loud enough it is deadened by passing through the screen; but if it is loud enough it sets the bells vibrating, and is heard afterwards. Now just notice this consequence. We have supposed a screen made out of these structures that I have imagined to represent atoms, and when I sing through the scale at one end of the room certain notes appear to be deadened. If I take away half of those structures, what will be the effect? Exactly the same notes will be deadened, but they will not be deadened so much; the notes which are picked out of the thinner screen to be deadened will be exactly the same notes, but the amount of the deadening will not be the same.

So far we have only been talking about the transmission of *sound*. You know that sound consists of certain waves which are passed along in the air; they are called "aerial vibrations." Now we also know that light consists of certain waves which are passed along not in the air, but along another medium. I cannot stop at present to explain to you what the sort of evidence is upon which that assertion rests, but it is the same sort of evidence as that which I shall try to show you belongs to the statement about atoms; that is to say, the "undulatory theory," as it is called, of light; the theory that light consists of waves transmitted along a certain medium, has passed out of the stage of being a theory, and has passed into the stage of being a demonstrated fact. The difference between a theory and a demonstrated fact is something like this: If you supposed a man to have walked from Chorlton Town Hall down here say in ten minutes, the natural conclusion would be that he had walked along the Stretford Road. Now that theory would entirely account for all the facts, but at the same time the facts would not be proved by it. But suppose it happened to be winter time, with snow on the road, and that you could trace the man's footsteps all along the road, then you would know that he had walked along that way. Now the sort of evidence we have to show that light does consist of waves transmitted through a medium is the sort of evidence that footsteps upon the snow make; it is not a theory merely which simply accounts for the facts, but it is a theory which can be reasoned back to from the facts without any other theory being possible. So that you

must just for the present take it for granted that the arguments in favour of the hypothesis that light consists of waves are such as to take it out of the region of hypothesis, and make it into demonstrated fact.

Very well, then, light consists of waves transmitted along this medium in the same way that sound is transmitted along the air. The waves are not of the same kind; but still they are waves, and they are transmitted as such; and the different colours of light correspond to the different lengths of these waves, or to the different rates of the vibration of the medium, just as the different pitches of sound correspond to the different lengths of the air waves, or to the different rates of the vibration of the air. Now if we take any gas, such as oxygen, and we pass light through it, we find that that gas intercepts, or weakens, certain particular colours. If we take any other gas, such as hydrogen, and pass light through it, we find that that gas intercepts, or weakens, certain other particular colours of the light. Now, there are two ways in which it can do that: it is clear that the undulations, or waves, are made weaker, because they happen to coincide with the rate of vibration of the gas they are passing through. But the gas may vibrate as a whole in the same way that the air does when you transmit sound. Or the waves may be stopped, because the gas consists of a number of small structures; just as my screen, which I imagine to consist of structures; or just as the screen of piano strings is made up of the same structure many times repeated. Either of these suppositions would apparently at first account for the fact that certain waves of light are intercepted by the gas, while others are let through. But now how is it that we can show one of these suppositions is wrong and the other is right? Instead of taking so small a structure as piano strings, let us suppose we had got a series of fiddles, the strings of all of them being stretched exactly in tune. I suppose this case because it makes a more complicated structure, for there would be two or three notes corresponding in each fiddle. If you suppose this screen of fiddles to be hung up and then compressed, what will be the effect? The effect of the compression will be, if they are all in contact, that each fiddle itself will be altered. If the fiddles are compressed longways, the strings will give lower notes than before, and consequently the series of notes which will be intercepted by that screen will be different from the series of notes which were intercepted before. But if you have a screen made out of fiddles which are at a distance from one another, and then if you compress them into a smaller space by merely bringing

them nearer together, without making them touch, then it is clear that exactly the same notes will be intercepted as before; only, as there will be more fiddles in the same space, the deadening of the sound will be greater.

Now when you compress any gas you find that it intercepts exactly the same colours of light which it intercepted before it was compressed. It follows, therefore, that the rates of vibration which it intercepts depend not upon the mass of the gas whose properties are altered by the compression, but upon some individual parts of it which were at a distance from one another before, and which are only brought nearer together without being absolutely brought into contact so as to squeeze them. That is the sort of reasoning by which it is made clear that the interception of light, or particular waves of light by means of a gas, must depend on certain individual structures in the gas which are at a distance from one another, and which by compression are not themselves compressed, but only brought nearer to one another.

There is an extremely interesting consequence which follows from this reasoning, and which was deduced from it by Professor Stokes in the year 1851, and which was afterwards presented in a more developed form in the magnificent researches of Kirchhoff—namely the reasoning about the presence of certain matter in the sun. If you analyse the solar light by passing it through a prism, the effect of the prism is to divide it off so as to separate the light into the different colours which it contains. That line of variously coloured light which is produced by the prism is, as you know, called the spectrum. Now when that spectrum is made in a very accurate way, so that the parts of it are well defined, it is observed to contain certain dark lines. That is, there is a certain kind of light which is missing in the sun light; certain kinds of light, as we travel along the scale of lights, are missing. Why are they missing? Because there is something that the light has passed through which intercepts or weakens those kinds of light. Now that something which the light has passed through, how shall we find out what it is? It ought to be the same sort of substance which if it were heated would give out exactly that kind of light. Now there is a certain kind of light which is intercepted which makes a group of dark lines in the solar spectrum. There are two principal lines which together are called the line D; and it is found that exactly that sort of light is emitted by sodium when heated hot enough. The conclusion therefore is that that matter which intercepts that particular part of the solar light is sodium, or that there is sodium somewhere

between us and the hot portion of the sun which sends us the light. And other reasons lead us to conclude that this sodium is not in the atmosphere of the earth, but in the neighbourhood of the sun—that it exists in a gaseous state in the sun's atmosphere. And nearly all the lines in the solar spectrum have been explained in that way, and shown to belong to certain substances which we are able to heat here, and to show that when they are heated they give out exactly the same kind of light which they intercepted when the light was first given out by the sun and they stood in the way. So you see that is a phenomenon exactly like the phenomenon presented by the finger-glass that we began with.

Precisely the same light which any gas will give out when it is heated, that same kind of light it will stop or much weaken it if the light is attempted to be passed through it. That means that this medium which transmits light, and which we call the "luminiferous ether," has a certain rate of vibration for every particular colour of the spectrum. When that rate of vibration coincides with one of the rates of vibration of an atom, then it will be stopped by that atom, because it will set the atom vibrating itself. If therefore you pass light of any particular colour through a gas whose atoms are capable of the corresponding rate of vibration, the light will be cut off by the gas. If on the other hand you so far heat the gas that the atoms are vibrating strongly enough to give out light, it will give out a light of a kind which it previously stopped.

We have reason then for believing that a simple gas consists of a great number of atoms; that it consists of very small portions, each of which has a complicated structure, but that structure is the same for each of them, and that these portions are separate, or that there is space between them.

In the next place I want to show you what is the evidence upon which we believe that these portions of the gas are in motion—that they are constantly moving.

If this were a political instead of a scientific meeting, there would probably be some people who would be inclined to disagree with us, instead of all being inclined to agree with one another; and these people might have taken it into their heads, as has been done in certain cases, to stop the meeting by putting a bottle of sulphuretted hydrogen in one corner of the room and taking the cork out. You know that after a certain time the whole room would contain sulphuretted hydrogen, which is a very unpleasant thing to come in contact with. Now how is it that that gas which was contained in a small bottle could get in a short time over the

whole room unless it was in motion? What we mean by motion is change of place. Now the gas was in one corner and it is afterwards all over the room. There has therefore been motion somewhere, and this motion must have been of considerable rapidity, because we know that there was the air which filled the room beforehand to oppose resistance to that motion. We cannot suppose that the sulphuretted hydrogen gas was the only thing that was in motion, and that the air was not in motion itself, because if we had used any other gas we should find that it would diffuse itself in exactly the same way. Now an argument just like that applies also to the case of a liquid. Suppose this room were a large tank entirely filled with water and anybody were to drop a little iodine into it, after a certain time the whole of the water would be found to be tinged of a blue colour. Now that drop may be introduced into any part of the tank you like, either at the top or bottom, and it will always diffuse itself over the whole water. There has here again been motion. We cannot suppose that the drop which was introduced was the only thing that moved about, because any other substance would equally have moved about. And the water has moved into the place where the drop was, because in the place where you put the drop there is not so much iodine as there was to begin with. Well then it is clear that in the case of a gas, these particles of which we have shown it to consist must be constantly in motion; and we have shown also that a liquid must consist of parts that are in motion, because it is able to admit the particles of another body among them.

Now when we have decided that the particles of a gas are in motion, there are two things that they may do—they may either hit against one another, or they may not. Now it is established that they do hit against one another, and that they do not proceed along straight lines independent of one another. But I cannot at present explain to you the whole of the reasoning upon which that conclusion is grounded. It is grounded upon some rather hard mathematics. It was shown by Professor Clerk Maxwell that a gas cannot be a medium consisting of small particles moved about in all directions in straight lines, which do not interfere with one another, but which bound off from the surfaces which contain this medium. Supposing we had a box containing a gas of this sort. Well, these particles do not interfere with one another, but only rebound when they come against the sides of the box; then that portion of the gas will behave not like a gas but like a solid body. The peculiarity of liquids and gases is that they do not mind being bent and having their shape

altered. It has been shown by Clerk Maxwell that a medium whose particles do not interfere with one another would behave like a solid body and object to be bent. It was a most extraordinary conclusion to come to, but it is entirely borne out by the mathematical formulæ. It is certain that if there were a medium composed of small particles flying about in all directions and not interfering with one another, then that medium would be to a certain extent solid, that is, would resist any bending or change of shape. By that means then it is known that these particles do run against one another. Now they come apart again. There were two things of course they might do, they might either go on in contact, or they might come apart. Now we know that they come apart for this reason—we have already considered how two gases in contact will diffuse into one another. If you were to put a bucket containing carbonic acid (which is very heavy) upon the floor of this room it would after a certain time diffuse itself over all the room; you would find carbonic acid gas in every part of the room. Now Graham found that if you were to cover over the top of that bucket with a very thin cover made out of graphite, or blacklead, then the gas would diffuse itself over the room pretty nearly as fast as before. The graphite acts like a porous body, as a sponge does to water, and lets the gas get through. The remarkable thing is that if the graphite is thin the gas will get through nearly as fast as it will if nothing is put between to stop it. Graham found out another fact. Suppose that bucket to contain two very different gases, say a mixture of hydrogen and carbonic acid gas. Then the hydrogen would come out through the blacklead very much faster than the carbonic acid gas. Now it is found by mathematical calculation that if you have two gases, which are supposed to consist of small particles which are all banging about, the gas whose particles are lightest will come out quickest; that a gas which is four times as light will come out twice as fast; and a gas nine times as light will come out three times as fast, and so on. Consequently, when you mix two gases together and then pass them through a thin piece of blacklead, the lightest gas comes out quickest, and is as it were sifted from the other. Now suppose we put pure hydrogen into a bucket and put blacklead on the top, and then see how fast the hydrogen comes out. If the particles of the hydrogen are different from one another, if some are heavier, the lighter ones will come out first. Now let us suppose we have got a vessel which is divided into two parts by a thin wall of blacklead. We will put hydrogen into one of these parts and allow it to come through this blacklead

into the other part; then if the hydrogen contains any molecules or atoms which are lighter than the others, those will come through first. If we test the hydrogen that has come through, we shall find that the atoms, as a rule, on one side of this wall are lighter than the atoms on the other side. How should we find that out? Why we should take these two portions of gas, and we should try whether one of them would pass through another piece of blacklead quicker than the other; because if it did, it would consist of lighter particles. Graham found that it did not pass any quicker. Supposing you put hydrogen into one half of such a vessel, and then allow the gas to diffuse itself through the blacklead, the gas on the two sides would be found to be of precisely the same qualities. Consequently, there has not been in this case any sifting of the lighter particles from the heavier ones; and consequently there could not have been any lighter particles to sift, because we know that if there were any they would have come through quicker than the others. Therefore we are led to the conclusion that in any simple gas, such as hydrogen or oxygen, all the atoms are, as nearly as possible, of the same weight. We have no right to conclude that they are exactly of the same weight, because there is no experiment in the world that enables us to come to an exact conclusion of that sort. But we are enabled to conclude that, within the limits of experiment, all the atoms af a simple gas are of the same weight. What follows from that? It follows that when they bang against one another, they must come apart again; for if two of them were to go on as one, that one would be twice as heavy as the others, and would consequently be sifted back. It follows therefore that two particles of a gas which bang against one another must come apart again, because if they were to cling together they would form a particle twice as heavy, and so this clinging would show itself when the gas was passed through the screen of blacklead.

Now there are certain particles or small masses of matter which we know to bang against one another according to certain laws; such, for example, as billiard balls. Now the way in which different bodies, after hitting together, come apart again depends on the constitution of those bodies. The earlier hypothesis about the constitution of a gas supposed that the particles of them came apart according to the same law that billiard balls do; but that hypothesis, although it was found to explain a great number of phenomena, did not explain them all. And it was Professor Clerk Maxwell again who found the hypothesis which does explain

all the rest of the phenomena. He found that particles when they come together separate as if they repelled one another, or pushed one another away; and as if they did that much more strongly when close together than when further apart. You know that what is called the great law of gravitation asserts that all bodies pull one another together according to a certain law, and that they pull one another more when close than when further apart. Now that law differs from the law which Clerk Maxwell found out as affecting the repulsion of gaseous particles. The law of attraction of gravitation is this; that when you halve the distance, you have to multiply the attraction four times—twice two make four. If you divide the distance into three, you must multiply the attraction nine times—three times three are 9. Now in the case of atomic repulsion you have got to multiply not twice two, or three times three, but five twos together—which multiplied make 32. If you halve the distance between two particles you increase the repulsion 32 times. So also five threes multiplied together make 243; and if you divide the distance between two particles by three, then you increase the repulsion by 243. So you see the repulsion increases with enormous rapidity as the distance diminishes. That law is expressed by saying that the repulsion of two gases is inversely as the fifth power of the distance. But now I must warn you against supposing that that law is established in the same sense that these other statements that we have been making are established. That law is true provided that there is a repulsion between two gaseous particles, and that it varies as a power of the distance; it is proved that if there is any law of repulsion, and if the law is that it varies as some power of the distance, then that power cannot be any other than the fifth. It has not been shown that the action between the two particles is not something perhaps more complicated than this, but which on the average produces the same results. But still the statement that the action of gaseous molecules upon one another can be entirely explained by the assumption of a law like that, is the newest statement in physics since the law of gravitation was discovered. You know that there are other actions of matter which apparently take place through intervening spaces and which always follow the same law as gravitation, such as the attraction or repulsion of magnetical or electrical particles: those follow the same law as gravitation. But here is a law of repulsion which follows a different law to that of gravitation, and in that lies the extreme interest of Professor Clerk Maxwell's investigation.

Now the next thing that I want to give you reasoning for is again rather a hard thing in respect of the reasoning, but the fact is an

extremely simple and beautiful one. It is this. Suppose I have two vessels, say cylinders, with stoppers which do not fit upon the top of the vessel, but slide up and down inside and yet fit exactly. These two vessels are of exactly the same size; one of them contains hydrogen and the other contains oxygen. They are to be of the same temperature and pressure, that is to say they will bear exactly the same weight on the top. Very well, these two vessels having equal volumes of gas of the same pressure and temperature will contain just the same number of atoms in each, only the atoms of oxygen will be heavier than the atoms of hydrogen. Now how is it that we arrive at that result? I shall endeavour to explain the process of reasoning. Boyle discovered a law about the dependence of the pressure of a gas upon its volume, which showed that if you squeezed a gas into a smaller space it will press so much the more as the space has been diminished. If the space has been diminished one-half, then the pressure is doubled; if the space is diminished to one-third, then the pressure is increased to three times what it was before. This holds for a varying volume of the same gas. That same law would tell us that if we put twice the quantity of gas into the same space, we should get twice the amount of pressure. Now Dalton made a new statement of that law, which expresses it in this form, that when you put more gas into a vessel which already contains gas, the pressure that you get is the sum of the two pressures which would be got from the two gases separately. You will see directly that that is equivalent to the other law. But the importance of Dalton's statement of the law is this, that it enabled the law to be extended from the case of the same gas to the case of two different gases. If instead of putting a pint of oxygen into a vessel already containing a pint, I were to put in a pint of nitrogen, I should equally get a double pressure. The oxygen and nitrogen when mixed together would exert the sum of the pressures upon the vessel that the oxygen and nitrogen would exert separately. Now the explanation of that pressure is this. The pressure of the gas upon the sides of the vessel is due to the impact of these small particles which are constantly flying about and impinging upon the sides of the vessel. It is first of all shown mathematically that the effect of that impinging would be the same as the pressure of the gas. But the amount of the pressure could be found if we knew how many particles there were in a given space, and what was the effect of each one when it impinged on the sides of the vessel. You see directly why it is that putting twice as many particles. which are

going at the same rate, into the same vessel, we should get twice the effect. Although there are just twice as many particles to hit the sides of the vessel, they are apparently stopped by each other when they bound off. But the effect of there being more particles is to make them come back quicker; so that altogether the number of impacts upon the sides of the vessel is just doubled when you double the number of particles. Now supposing we have got a cubic inch of space, then the amount of pressure upon the side of that cubic inch depends upon the number of particles inside the cube, and upon the energy with which each one of them strikes against the sides of the vessel.

Well now again there is a law which connects together the pressure of a gas and its temperature. It is found that there is a certain absolute zero of temperature, and that if you reckon your temperature from that then the pressure of the gas is directly proportional to the temperature, that twice the temperature will give twice the pressure of the same gas, and three times the temperature will give three times the pressure of the same gas.

Well now we have just got to remember these two rules—the law of Boyle, as expressed by Dalton, connecting together the pressure of a gas and its volume, and this law which connects together the pressure with the absolute temperature. You must remember that it has been calculated by mathematics that the pressure upon one side of a vessel of a cubic inch has been got by multiplying together the number of particles into the energy with which each of them strikes against the side of the vessel. Now if we keep that same gas in a vessel and alter its temperature, then we find that the pressure is proportional to the temperature; but since the number of molecules remains the same when we double the pressure, we must alter that other factor in the pressure, we must double the energy with which each of the particles attacks the side of the vessel. That is to say, when we double the temperature of the gas we double the energy of each particle; consequently the temperature of the gas is proportional always to the energy of its particles. That is the case with a single gas. If we mix two gases, what happens? They come to exactly the same temperature. It is calculated also by mathematics that the particles of one gas have the same effect as those of the other; that is, the light particles go faster to make up for their want of weight. If you mix oxygen and hydrogen, you find that the particles of hydrogen go four times as fast as the particles of oxygen. Now we have here a mathematical statement—that when two gases are mixed together, the energy of the two particles

is the same; and with any one gas considered by itself that energy is proportional to the temperature. Also when two gases are mixed together the two temperatures become equal. If you think over that a little you will see that it proves that whether we take the same gas or different gases, the energy of the single particles is always proportional to the temperature of the gas.

Well now what follows? If I have two vessels containing gas at the same pressure and the same temperature (suppose that hydrogen is in one and oxygen in the other) then I know that the temperature of the hydrogen is the same as the temperature of the oxygen, and that the pressure of the hydrogen is the same as the pressure of the oxygen. I also know (because the temperatures are equal) that the average energy of a particle of the hydrogen is the same as that of a particle of the oxygen. Now the pressure is made up by multiplying the energy by the number of particles in both gases; and as the pressure in both cases is the same, therefore the number of particles is the same. That is the reasoning; I am afraid it will seem rather complicated at first hearing, but it is this sort of reasoning which establishes the fact that in two equal volumes of different gases at the same temperature and pressure, the number of particles is the same.

Now there is an exceedingly interesting conclusion which was arrived at very early in the theory of gases, and calculated by Mr. Joule. It is found that the pressure of a gas upon the sides of a vessel may be represented quite fairly in this way. Let us divide the particles of gas into three companies or bands. Suppose I have a cubical vessel in which one of these companies is to go forward and backward, another right and left, and the other to go up and down. If we make those three companies of particles to go in their several directions, then the effect upon the sides of the vessel will not be altered; there will be the same impact and pressure. It was also found out that the effect of this pressure would not be altered if we combined together all the particles forming one company into one mass, and made them impinge with the same velocity upon the sides of the vessel. The effect of the pressure would be just the same. Now we know what the weight of a gas is, and we know what the pressure is that it produces, and we want to find the velocity it is moving at on the average. We can find out at what velocity a certain weight has got to move in order to produce a certain definite impact. Therefore we have merely got to take the weight of the gas, divide it by three, and to find how fast that has got to move in order to produce the pressure, and that will give us the average rate at

which the gas is moving. By that means Mr. Joule calculated that in air of ordinary temperature and pressure the velocity is about 500 metres per second, nearly five miles in sixteen seconds, or nearly twenty miles a minute--about sixty times the rate of an ordinary train.

The average velocity of the particles of gas is about $1\frac{1}{2}$ times as great as the velocity of sound. Now you can easily remember the velocity of sound in air at freezing point—it is 333 metres per second; so that about $1\frac{1}{2}$ times, really 1·432 of that would be the average velocity of a particle of air. At the ordinary temperature—60 degrees Fahrenheit—the velocity would, of course, be great

Now then just let us consider how much we have established so far about these small particles of which we find that the gas consists. We have so far been treating mainly of gases. We find that a gas, such as the air in this room, consists of small particles, which are separate with spaces between them. They are as a matter of fact of two different types, oxygen and nitrogen. All the particles of oxygen contain the same structure, and the rates of internal vibration are the same for all these particles. It is also compounded of particles of nitrogen which have different rates of internal vibration. We have shown that these particles are moving about constantly. We have shown that they impinge against and interfere with one another's motion; and we have shown that they come apart again. We have shown that in vessels of the same size containing two different gases of the same pressure and temperature there is the same number of those two different sorts of particles. We have shown also that the average velocity of these particles in the air of this room is about twenty miles a minute.

Now there is one other point of very great interest to which I want to call your attention. The word "atom," as you know, has a Greek origin; it means—that which is not divided. Various people have given it the meaning of that which *cannot* be divided; but if there is anything which cannot be divided we do not know it, because we know nothing about possibilities or impossibilities, only about what has or has not taken place. Let us then take the word in the sense in which it can be applied to a scientific investigation. An atom means something which is not divided *in certain cases that we are considering*. Now these atoms I have been talking about may be called physical atoms, because they are not divided under those circumstances that are considered in physics. These atoms are not divided under the ordinary alteration of temperature and pressure of gas, and

variation of heat; they are not in general divided by the application of electricity to the gas, unless the stream is very strong. But there is a science which deals with operations by which these atoms which we have been considering can be divided into two parts, and in which therefore they are no longer atoms. That science is chemistry. The chemist therefore will not consent to call these little particles that we are speaking of by the name of atoms, because he knows that there are certain processes to which he can subject them which will divide them into parts, and then they cease to be things which have not been divided. Now I will give you an instance of that. The atoms of oxygen which exist in enormous numbers in this room consist of two portions, which are of exactly the same structure. Every molecule, as the chemist would call it, travelling in this room, is made up of two portions which are exactly alike in their structure. It is a complicated structure; but that structure is double. It is like the human body—one side is like the other side. How do we know that? We know it in this way. Suppose that I take a vessel which is divided into two parts by a division which I can take away. One of these parts is twice as large as the other part, and will contain twice as much gas. Into that part which is twice as big as the other I put hydrogen; into the other I put oxygen. Suppose that one contains a quart and the other a pint; then I have a quart of hydrogen and a pint of oxygen in this vessel Now I will take away the division so that they can permeate one another, and then if the vessel is strong enough I pass an electric spark through them. The result will be an explosion inside the vessel; it won't break if it is strong enough; but the quart of hydrogen and the pint of oxygen will be converted into steam; they will combine together to form steam. If I choose to cool down that steam until it is just as hot as the two gases were before I passed the electric spark through them, then I shall find that at the same pressure there will only be a quart of steam. Now let us remember what it was that we established about two equal volumes of different gases at the same temperature and pressure. First of all, we had a quart of hydrogen with a pint of oxygen. We know that that quart of hydrogen contains twice as many hydrogen molecules as the pint of oxygen contains of oxygen molecules. Let us take particular numbers. Suppose instead of a quart or a pint we take a smaller quantity, and say that there are 100 hydrogen and 50 oxygen molecules. Well after the cooling has taken place, I should find a volume of

steam which was equal to the volume of hydrogen, that is I should find 100 steam molecules. Now these steam molecules are made up of hydrogen and oxygen molecules. I have got therefore 100 things which are all exactly alike, made up of 100 things and 50 things—100 hydrogen and 50 oxygen, making 100 steam molecules. Now since the 100 steam molecules are exactly alike, we have those 50 oxygen molecules distributed over the whole of these steam molecules. Therefore unless the oxygen contains something which is common to the hydrogen also, it is clear that each of those 50 molecules of oxygen must have been divided into two; because you cannot put 50 horses into 100 stables, so that there shall be exactly the same amount of horse in each stable; but you can divide 50 *pairs* of horses among 100 stables. There we have the supposition that there is nothing common to the oxygen and hydrogen, that there is no structure that belongs to each of them. Now that supposition is made by a great majority of chemists. Sir Benjamin Brodie, however, has made a supposition that there is a structure in hydrogen which is also common to certain other elements. He has himself, for particular reasons, restricted that supposition to the belief that hydrogen is contained as a whole in many of the other elements. Let us make that further supposition and it will not alter our case at all. We have then one hundred hydrogen and fifty oxygen molecules, but there is something common to the two. Well this something we will call X. Of this we have to make one hundred equal portions. Now that cannot be the case unless that structure occurred twice as often in each molecule of oxygen as in each molecule of hydrogen. Consequently, whether the oxygen molecule contains something common to hydrogen or not, it is equally true that the oxygen molecule must contain the same thing repeated twice over; it must be divisible into two parts which are exactly alike.

Similar reasoning applies to a great number of other elements; to all those which are said to have an even number of atomicities. But with regard to those which are said to have an odd number, although many of these also are supposed to be double, yet the evidence in favour of that supposition is of a different kind; and we must regard the supposition as still a theory and not yet a demonstrated fact.

Now I have spoken so far only of gases. I must for one or two moments refer to some calculations of Sir Wm. Thompson, which are of exceeding interest as showing us what is the proximity of the molecules in liquids and in solids. By four different modes

of argument derived from different parts of science, and pointing mainly to the same conclusion, he has shown that the distance between two molecules in a drop of water is such that there are between five hundred millions and five thousand millions of them in an inch. He expresses that result in this way—that if you were to magnify a drop of water to the size of the earth, then the coarseness of the graining of it would be something between that of cricket balls and small shot. Or we may express it in this rather striking way. You know that the best microscopes can be made to magnify from 6,000 to 8,000 times. A microscope which would magnify that result as much again would show the molecular structure of water.

There is another scientific theory analogous to this one which leads us to hope that some time we shall know more about these molecules. You know that since the time that we have known all about the motions of the solar system, people have speculated about the origin of it; and a theory started by Laplace and worked out by other people has, like the theory of luminiferous ether, been taken out of the rank of hypothesis into that of fact. We know the rough outlines of the history of the solar system, and there are hopes that when we know the structure and properties of a molecule, what its internal motions are and what are the parts and shape of it, somebody may be able to form a theory as to how that was built up and what it was built out of. It is obvious that until we know the shape and structure of it, nobody will be able to form such a theory. But we can look forward to the time when the structure and motions in the inside of a molecule will be so well known that some future Kant or Laplace will be able to form a hypothesis about the history and formation of matter.

In acknowledging a vote of thanks, Professor Clifford took the opportunity of recommending his auditors to read Professor Clerk Maxwell's book on the Theory of Heat, at the end of which would be found a short exposition of the molecular theory of matter.

NOTE.—*The mathematical development of this subject is due to Clausius and Maxwell. References to the chief papers will be found at the beginning of Maxwell's memoir "On the Dynamical Theory of Gases," Phil. Trans.*, 1867.

FLAME.

A LECTURE

BY PROFESSOR CORE,

OF OWENS COLLEGE, MANCHESTER,

Delivered in the Hulme Town Hall, Manchester, Nov. 27th, 1872.

THE subject that I have to bring before your notice this evening may seem a very simple one, and possibly a vague wonder may fill the minds of many of you as to what is the use of all this apparatus to illustrate such a familiar and commonplace subject. But I trust that before the lecture is finished, you will have found that there is a great deal of interesting information to be derived from a careful study of the very familiar phenomenon of a gas flame.

As I shall have occasion frequently to make use of the word "combustion," I think it is as well in the first place to explain clearly what is meant by this term. In the first place I shall show you what it is not. I take here a piece of platinum wire coiled into a spiral, and I hold this in the flame of a gas lamp, and you see it becomes white hot. This body, however, is not in combustion. When I withdraw it from the flame, it immediately loses both its heat and its light. This platinum coil is not burned---it is not in combustion; it is, as we say, in ignition, or it is incandescent.

I shall now take a more intense flame, that produced by the oxy-hydrogen blow pipe, which is one of the most intense kinds of heat that we can produce. I shall insert in this a piece of lime, and we shall see also the lime glowing or ignited, but at the same time not in combustion. [Experiment.] In this case also the lime was incandescent or ignited, but it was not in combustion. If, however, I put a piece of wood in the flame of the lamp, then we have a totally different phenomenon. When I withdraw tne

wood the action is still maintained. We see besides that there is a bodily dispersion of the substance of the wood; whereas in the former case of the platinum, and in the case of the lime, if we weigh the body immediately afterwards we find that it has lost no weight. We see, therefore, that in ignition there is no loss of weight, whereas in combustion there is an apparent loss of weight. I shall take a piece of charcoal and insert this also in the same flame—the oxy-hydrogen flame. You see that it not only emits a strong light, but at the same time it keeps glowing, and will maintain its high temperature for a long time after it has been removed from the source of heat. I now hold a piece of magnesium wire in the flame of this gas lamp, and you see that it is not only ignited, but it is actually burning. We have in this case the magnesium, not incandescent, but in combustion. These examples, therefore, are sufficient to show the fundamental difference between these two states.

Now let us consider a little more minutely this phenomenon of combustion. I have already said that there is an apparent loss of weight. However, when we examine it more minutely, we find that the body which is burning is silently combining with the oxygen of the air. I take it for granted you all know that the atmosphere which we breathe consists of about one-fifth of its bulk of oxygen, and it is this oxygen which bestows upon the atmosphere its positive properties, by which it is enabled to support the respiration of animals and the life of plants. Well, this oxygen during the process of combustion is silently but constantly uniting with the body which is being burned; so that we have a new substance produced, a substance, namely, which is a mixture of oxygen plus the burning body. When I burn carbon or charcoal, therefore, in common air, we have as the product of that combustion a mixture of carbon and oxygen which is called carbonic acid. Of course, the product that is formed will depend upon the particular combustible that is made use of. Now, I have already mentioned that it is the oxygen which is thus combined with the combustible body. If we had an atmosphere of pure oxygen, then the process of combustion would go on much more rapidly, in the same way that the process of respiration would go on much more rapidly; in fact it would go on too rapidly for our frames. If we inhale a few mouthfuls of pure oxygen it has an exhilarating effect upon the system; but if the process is continued too long it induces fever; and small animals, such as mice or rabbits, when plunged into a jar containing pure oxygen, in a very short time are killed by its too violent action. So that it is necessary in the

atmosphere to have the positive property of oxygen diluted, as it were, with four times its bulk of a neutral gas—nitrogen. I shall now show you that in oxygen the process of combustion goes on much more vividly. We have here three large glass globes which are filled with oxygen gas. Of course the gas is invisible, but they have been filled beforehand. We now invert one of the jars over a little cup which contains phosphorus, and another is inverted over a cup which contains sulphur: and the globe nearest me is inverted over pieces of charcoal. The whole are connected by wires proceeding from a battery under the table, and when the contact is made, the current flying along the wire will raise to a red heat the small piece of platinum wire passing over these combustible bodies. You see the phosphorus is beginning to burn first; and you see how vividly the process of combustion goes on in an atmosphere of pure oxygen. [The experiments were brilliant, and, like the succeeding experiments, were successful and much applauded.] I shall now show you that when we heat iron and play on it with oxygen we can even burn solid iron. Here are a number of common nails put into a cavity scooped out in a firebrick, where we are heating them, and when heated to a sufficient temperature the pure oxygen will be turned on. [Experiment, with the emission of countless brilliant sparks.] Thus you see solid iron burned. Here is another example of a piece of iron plate which we will show you can be burned right through by the same agency. You see that a small piece of iron has been burned out of this plate of metal by the oxygen. These examples therefore are sufficient to show that in an atmosphere of pure oxygen the process of combustion would go on too rapidly.

Now some substances require a much higher temperature to unite with oxygen than others do. The process of combination may go on at all temperatures; but it is not the mere combination with oxygen that produces combustion. If we expose a piece of bright pure iron to the atmosphere, we observe that in a short time it becomes covered with rust. Well, the action is much the same in this case, for we have the iron uniting with the oxygen to form a new body; in fact, the rust that is formed is a definite product of iron and oxygen, and is called an oxide of iron. Here the iron unites at a very low temperature with the oxygen of the air. In this case heat is given out, as in all cases where chemical union between two bodies takes place. Of course the amount of heat is necessarily very small; but it can be estimated, and it is found to be invariably constant, that is, the same weight

of iron will always, in uniting with oxygen, give out precisely the same amount of heat; and, of course, if we spread the operation over a long time, the intensity of the heat is so much the less, and by making the iron combine much more rapidly with the oxygen, we thereby diminish the time in which the action takes place, and consequently we increase the intensity of the resulting action.

Oxygen, therefore, is a body whose presence is essential in all cases of combustion. There are other bodies which are commonly called combustible, but erroneously, because oxygen is as much combustible as the other bodies to which we are about to refer. Of our other combustible bodies I shall take as specimens hydrogen and carbon, because these are the principal ingredients in all our common burning bodies, such as common gas, candles, the oil of lamps, and the coal of our fires; in fact, all our ordinary sources of artificial light are derived either from carbon or hydrogen, or a mixture of the two. Now hydrogen, in uniting with oxygen, generates steam; that is to say, water is a definite combination of oxygen and hydrogen; so that in every case of combustion, where hydrogen is one of the bodies and oxygen the other, we find that the product of the combustion takes the form of steam or water. I shall now show that when hydrogen is burned water is formed. I have here a large bell jar, which you will observe is perfectly clear; there is no dimness upon it. The glass will be held over the burning hydrogen, and in a short time you will notice it is bedimmed with dew. You see at once that it has become dim. Now, if we were to hold it long enough over the burning hydrogen, this dimness would increase, and ultimately the little particles of moisture would unite and form drops of water. We see, therefore, that steam, or water, which is simply steam in a different form, is a compound of oxygen and hydrogen, and is always therefore a necessary product of combustion when these two bodies burn together. Now, in what proportions are the two mixed in order to form water? In the proportion of one part by weight of hydrogen to eight of oxygen. If we mix the two gases in this proportion we can then, instead of burning the hydrogen silently, burn it as it were instantaneously, and an explosion is the result. Here is a small glass bulb, which has been previously filled with a mixture of these two gases derived from the decomposition of water. The water was resolved by means of the voltaic current into its two elements, and the two gases that resulted from the decomposition were received into this bulb. You notice these two upright wires proceeding from our battery, and connected by a short platinum

wire, which you see is raised to a white heat when I make contact. I now uncork the bulb, invert it over the wires, and make contact. [The first trial of this experiment failed, in consequence of the intense heat having melted the wire.] We will replace it by a fresh wire. [A loud explosion, the glass bulb being shivered into a thousand fragments.]

A characteristic property of hydrogen is its extreme lightness; it is one of the lightest bodies that we know, being only the fifteenth of the weight of its bulk of air. If a balloon is filled with pure hydrogen, its buoyancy is sufficient to overcome the weight of the envelope, and the balloon rises very rapidly. In practice large balloons are not filled with this gas, as its preparation on a large scale is attended with considerable expense; but a cheaper, though considerably heavier substitute, coal gas, is used. Here are two balloons which have been filled with hydrogen gas. [The balloons, when released, rose rapidly to the ceiling and remained there]. The mouths of these balloons have been purposely left a little open, so that the hydrogen may gradually escape; and, of course, as it escapes its place is filled by air, and consequently the balloons will gradually descend. Having now considered some of the properties of hydrogen, let us now examine carbon, which is one of the commonest combustible bodies that we find around us.

A piece of charcoal, for example, is almost pure carbon. Plumbago or blacklead is another form of carbon. Now, when carbon is united with oxygen, we have two products formed—carbonic acid is one, and carbonic oxide is the other. These two, however, differ in the amount of oxygen which they contain. If we burn charcoal in a limited supply of oxygen, we have carbonic oxide produced, where we have one equivalent of carbon to one of oxygen. But if we burn carbon in a plentiful supply of oxygen, so that the carbon can unite with as much oxygen as it pleases, we have carbonic acid produced, which contains twice as much oxygen as the other compound does. I have got a jar here which contains carbonic acid, and you will observe that it is perfectly colourless. However, it has a great many very active properties. It at once extinguishes combustion, so that if I were to plunge this lighted taper into it, the taper would be extinguished. I shall pour this invisible gas from one glass jar into another with a lighted taper at the bottom: you will see nothing pass, but you will notice that it extinguishes the flame. [Experiment.] You see the flame is at once extinguished. I shall now put the flame into a jar which contains both oxygen and carbonic acid. We have oxygen in the upper part,

and carbonic acid in the lower part of the jar. In a short time these two gases would mix, but at present they occupy different positions in the vessel. The flame is now in the oxygen, and burns brightly; it is now in the carbonic acid, and is immediately extinguished. This experiment requires to be done very quickly, because however different the densities of the two gases are they soon mix. We see from these experiments another property of carbonic acid—its great weight; for it admits of being poured like water from one vessel into another, and lies for a time at the bottom of a vessel with oxygen above it. With all gases that are lighter than common air we have to invert the process, and pour the light gas upwards. Now as carbonic acid is destructive of combustion it is equally fatal to the respiration of animals; a few inhalations of the pure gas would bring on insensibility, and if the process were continued for two or three minutes death would be the result. As this gas is a constant product of combustion, you see it is necessary in cases where combustion is going on that there should be a free ventilation in order to carry off this noxious product. I daresay many of you have read of melancholy instances of persons falling asleep in ill-ventilated rooms with a stove or fireplace filled with burning charcoal, and never awaking. They were poisoned by the carbonic acid. Carbonic oxide, as I have said, is produced where there is a limited supply of oxygen. You may frequently have observed in a common fireplace the upper part of the fire burning with a pale blue flame. This is the flame of carbonic oxide, which is therefore very different from carbonic acid. The one is incombustible and extinguishes flame; the other burns with a bluish flame. I have here a jar of carbonic oxide, and on applying a lighted taper to it we shall find that instead of extinguishing the taper, the gas itself burns. [Experiment.] In a common fireplace the carbonic oxide is formed thus—the oxygen of the air entering the lower part of the fire and there meeting with the glowing coals, the carbon of the coals and the oxygen form carbonic acid. As this gas ascends through the burning charcoal which lies above, it is deprived of part of its oxygen and is converted into the lower oxide or carbonic oxide which burns with a pale blue flame in the upper part of the fireplace. This blue flame frequently appears of a purplish tint, as it is often seen on a red background of hot brick.

We have now considered separately hydrogen and carbon, which are the burning bodies; and oxygen, which is said to be the supporter of combustion, although I prefer to designate all

three by the same title, and call them all combustible bodies, because the process of combustion is simply a process of union. So that combustion may be defined to be chemical action in which so much heat is given out as to produce light. If, now, our burning body be a gas—in other words, if we have a gas in this process of uniting with oxygen, then flame is the result. So that we may define flame to be gas in a state of combustion.

Now, I shall take coal gas as our typical gas. I shall fill this large wide-mouthed hollow globe partly with coal gas and partly with common air. At present it is full of air, the same as we have in this room; but I shall introduce for a few seconds the open end of this flexible tube which communicates with the gas pipes. I drop a piece of lighted paper into the vessel, and I wish you to observe the nature of the resulting flame. [Pale, but loud-roaring flame.] You observe that instead of burning in the ordinary and quiet way, and giving out a great amount of light, it burns rapidly, and gives out very little light. We see, therefore, that the manner in which gas burns depends a good deal on the manner in which we mix the oxygen of the air with it. Now when we begin to analyse coal gas, and try to find out of what ingredients it consists, we find that it is a very complex substance; but that the valuable ingredients are hydrocarbons, or compounds of carbon and hydrogen. Olefiant gas is the most valuable constituent in coal gas; and the more we have of this olefiant gas—or as it is sometimes called heavy carburetted hydrogen—the higher is the illuminating power. Here is a jar of olefiant gas; you notice the flame with which it burns. [Experiment.] It burns with a dense white flame. I shall afterwards have to explain why in this case we had a great deal of light, and in the former very little. This olefiant gas is a compound of carbon and hydrogen, in the proportion of one part by weight of carbon to two of hydrogen. I can demonstrate the presence of solid carbon in this invisible gas. I have got here two jars, one contains olefiant gas and the other chlorine gas. Now, I shall allow these two gases to mix, by placing the open mouth of the one vessel over that of the other. I shall now apply a light to the mixture, and you will observe that the burning of the mixture is attended by the deposition of a large quantity of soot. [A tall column of dense smoke and soot was produced.] Here we have the carbon distinctly visible, and it comes only from the olefiant gas, because the other gas, the chlorine, contains not a particle of carbon. When olefiant gas is mixed with oxygen in the proportion of one part gas by measure to

three of oxygen, an explosive compound is formed. Mr. Harrison will show us some soap bubbles filled with this mixture and explode them. I have here got a small flask filled with the mixture, and I will explode it by applying a light to it. Mr. Harrison has here made a large cauliflower-like head of many soap bubbles in a saucer, and we will explode the lot. [All loud explosions.] This olefiant gas whose properties we have been considering is that constituent in coal gas to which, as I have said, the latter owes its illuminating power. Generally, in good gas, there is about one-fifth part by volume of this gas present. There is a larger volume, however, of another compound of carbon and hydrogen, called marsh gas, or light carburetted hydrogen, the proportion being from two to two and a half, and sometimes three times the volume of the heavy carburetted hydrogen. This marsh gas, as its name implies, is frequently formed in stagnant water, if decaying vegetable matter is at the bottom of the pool. You have I dare say frequently noticed bubbles of gas rising in these stagnant pools and bursting at the top. These are bubbles of marsh gas or light carburetted hydrogen. I have got here a jar of this gas, and I will burn it. You observe that it burns with a whitish flame, much paler than the corresponding flame of the heavy carburetted hydrogen. Now this marsh gas is not only formed from decaying vegetable matter at the bottom of stagnant pools, but it is frequently formed in large quantities in the seams of coal mines, and it forms then what miners call "fire damp." Frequently in coal mines this gas is generated in enormous quantities, and when a reservoir of it is tapped the gas issues in great volume and with great velocity from the orifice, forming what miners call a "blower." Now, if this gas is mixed with about twice its volume of oxygen, or ten volumes of common air, it forms an explosive mixture. Consequently we can understand how it is that dreadful explosions sometimes take place in coal mines. The dangerous fire-damp or marsh gas produced from the layers of coal becomes mixed with the atmosphere in the mines, and when the proper proportion is obtained, and a light applied to the mixture, it at once explodes; and the result of the explosion is that carbonic acid is generated; so though the unfortunate miners should escape being hurt by the explosion, they are in great danger of being poisoned by the carbonic acid which is thus formed, and which the miners call "choke-damp," and sometimes "after-damp." I shall by and by have to call your attention to the Davy lamp, which is an admirable precaution against the occurrence of these very dangerous explosions.

Let us now consider attentively this flame, which we know to be gas in a state of combustion. I shall suppose, for the sake of simplicity, that our gas is entirely composed of the two whose properties we have lately been examining, namely, the two carburetted hydrogens, light and heavy. The gas, as it is being conducted along the pipe to the lamp is, of course, not allowed to mix with the air, with which it comes in contact only when it has reached the opening of the burner. Here on applying heat we determine the union of the oxygen with the carbon and hydrogen. If each particle of carbon and hydrogen had its share of oxygen close beside it, the union of the whole would be instantaneous, and we should have very little light. The luminosity of the flame depends upon the fact that the process of union is not instantaneous but gradual. We find that in the middle of the flame there is pure gas; immediately surrounding this dark region we have a luminous envelope; and if the atmosphere of this room were perfectly still, we should see the flame with a definite conical shape, and on the extreme border we should notice a very pale bluish flame of great heat but of very little light. So that we distinguish in the flame these three regions —a central region of no combustion, where we have almost unmixed gas; a middle or luminous region; and an outer or non-luminous but very hot region. I now show you that in the middle of this flame we have unconsumed gas. I insert the end of an open glass tube in the middle of the flame, and I allow the gas to rise up the tube. I now set fire to it at the other end; and you see it burning brightly. This experiment proves that the oxygen had not mixed with the gas in the centre of the flame; in fact the burning gas on the outside prevents the oxygen from getting access to it. In the central layer the oxygen of the air has already arrived, but not in sufficient quantity to satisfy both the hydrogen and carbon. Of the two bodies hydrogen most readily unites with oxygen, so that it is burned before the carbon is consumed. Now you have already seen that when hydrogen and oxygen burn they give out great heat but very little light; but the great heat resulting from this combustion heats the particles of carbon to a white heat. And in exactly the same way as when the oxy-hydrogen flame played upon the lime light we had intense light, so now when the union of the hydrogen and oxygen heats the little particles of carbon to a white heat, we have a luminous flame; so that the luminosity of the flame depends upon the fact that the hydrogen burns before the carbon. In the outer region where the oxygen has

ready access to both the hydrogen and carbon then we have the complete combustion of both the hydrogen and carbon. If I mix the gas with air before setting fire to it at the top, the character of the flame will be altered. The chimney of this gas lamp is provided at the bottom with a little cap, by turning which round four holes are opened, and consequently the air of the room is allowed to enter and mix with the gas as it rises in the chimney; and when the mixture is set fire to it burns with a different flame; in other words, the oxygen has been mixed with the gas, and the carbon and hydrogen are now burning simultaneously, consequently there are no carbon particles to be heated to a white heat, and the flame is feebly luminous. I have here a piece of wire gauze, and I wish to show you that the flame won't pass through it. I press the gauze down upon the flame, but it won't pass through. Now that there is gas above the gauze I can show by setting fire to it on the other side. You observe that the gas burns at the top, though the flame won't pass through. Not only so, but I can first allow the gas to pass through and ignite it at the top of the gauze, but the flame will not pass through to light the gas which we know to be below. This shows very distinctly that wire gauze serves as an obstacle, as it were, to prevent the passage of the flame through it. Now, Sir Humphrey Davy made use of this property of wire gauze in constructing his very ingenious and exceedingly useful safety lamp Here is a safety lamp of the ordinary construction. You will notice that it consists of a common oil lamp, with a cylindrical chimney of wire gauze, which will allow, of course, the oxygen of the air to pass through and maintain the combustion of the oil within, but, if there is a combustible gas on the outside, the flame cannot pass through to set fire to it.

Now suppose we have this protected flame in an atmosphere of marsh gas. The gas may pass through the little meshes of the gauze, and may burn inside the hollow cylinder; but the flame cannot pass outwards; so that we can carry with impunity this protected light through a combustible and even explosive atmosphere. I shall immerse the Davy lamp in an explosive gas, and you will see that the protected flame cannot pass out to set fire to the explosive gas on the outside. You already see that the gas has passed within and burns, but the flame cannot pass out to set fire to the gas on the outside. I shall now show you that there is combustible gas on the outside by setting fire to it. [Experiment.] So that the Davy lamp would serve as a perfect protection in mines, if only the miners would use it carefully. And in fact it

not only serves as a preventive of accidents, but it also serves as a warning of the existence of a dangerous gas in the mine; for as soon as a miner sees his lamp glowing within, he knows that some of the fire damp has passed within his lamp, and, consequently, that it is time for him to be leaving the mine. It is entirely through the recklessness of miners, who will persist in unscrewing the gauze of their lamps in order to light their pipes, that the melancholy accidents take place that we so frequently read about. I am speaking, of course, only of those accidents which arise from explosions owing to the use of naked lights.

We have now seen that when certain gases are mixed with a proper proportion of oxygen, the combustion is instantaneous, in other words, there is an explosion. Now I dare say most of you know that when a series of noises take place with great enough rapidity, a musical note is produced. If, now, I can mix a burning gas with a limited supply of oxygen, so as to make it explode; and if I can make these explosions follow each other sufficiently fast, we shall have a musical note produced; or, in other words, we shall have a musical flame. This musical flame was first observed about one hundred years ago by Dr. Higgins, a Dublin gentleman, who was burning hydrogen, and who found that when a small jet of burning hydrogen was protected in a measure from the external atmosphere by placing a glass tube over it open at both ends, the little hydrogen flame emitted a musical note. But it has been found that hydrogen is not the only gas which emits musical notes; in fact, any gas which admits of being burned can be made to emit a musical note.

Here is a small jet of coal gas burning at a common gas burner, which can be made to sing by placing a tube of glass over it. [Experiment.] The pitch of this note depends upon the length of the tube; so that if I take a longer tube, I shall get a graver note; but if I take a shorter tube, I shall get a higher note. I shall now place a much shorter tube over another flame, and you will hear a different tone produced. [Two notes of different pitch were now heard.] I have got a series of four pipes here of such lengths that when they are placed over their flames we have the common chord produced; that is to say we have one note a third above the first, another a fifth above it, and the fourth the octave of the first. I shall now place them all in position, and you will hear the common chord sounded. [The four notes of the common chord were heard distinctly.] By checking the supply of air, I can stop the singing of any flame. In this way it would be possible, theoretically at any rate, to construct

a musical instrument if we had a series of pipes arranged, with appliances to stop and open the tubes at will. If we examine attentively the condition of one of these singing flames we find that, unlike an ordinary flame, it does not burn steadily, but it is in a violent state of commotion—it is dancing up and down. And not only so, but it can be shown to you that the little singing flame is periodically extinguished; in fact, the whole explanation of this phenomena is, that a limited supply of oxygen having mixed with the air, an explosion succeeds immediately. The gas is again mixed with its proper dose of oxygen, relit by the intense heat, and another explosion takes place; so that we have these processes going on consecutively—the gas issuing from the orifice, mixing with its limited dose of oxygen, taking fire and exploding; again being mixed and again exploding, and so on, the whole operation going on many hundred times in a minute. I shall now show you on a larger scale that our singing flame is not only quivering violently, but at the same time is actually extinguished and lit alternately. The arrangement I have got for this is very simple. I have a large tube, in order to have as large a flame as possible; then by means of this concave mirror behind it I can reflect a magnified image of this flame upon a transparent screen in front of you; and you can see already, when the mirror is steady, that the flame is quivering. I now make the mirror revolve rapidly, and you notice that we have a series of tongues of fire, as it were, arranged along the screen. If you watch closely these different tongues, you will see an absolutely black interval separating the bright images. The explanation of this is that we have here an image, not of say a dozen jets which are burning at the same time, but an image of a dozen which were burning at different times. You must accept it as a fact that an impression of light received into the eye lasts for about the tenth part of a second; so that if the light of any object should fall ten times on the eye in a second, we have the impression of a continuous light. For example, when we twirl a string, with one extremity burning, quickly round, we see a continuous circle of fire. This is because the impression of the light has remained for a certain definite portion of time upon the retina. Precisely in the same way here, the image of each flame is thrown upon the screen, and the effect remains upon the retina of the eye for some little time, the series of flames succeeding each other so rapidly that we see the images of a considerable number of them upon the screen together; that is to say we have an image of about ten or twelve flames which exist actually at different portions

of time, though we see their images simultaneously. Now, as there are black intervals between these luminous tongues, it is evident that between the existence of one flame and the next there must have been a time when there was absolute darkness; in other words, our flame was burning and extinguished, burning and extinguished, and this, of course, so quickly that we have the impression of a continuous light.

I shall next show you that one of these singing flames can start another one into song, provided we have over it a tube which will be capable of resounding to the same note. I have here a small flame which can be made to sing in the usual way: then at some little distance from it I have a similar flame provided with a similar tube; but I adjust the tube over one so that it shall remain perfectly still. The moment I place the tube over the second flame it will begin to sing; and after a little it will shake its neighbour into song. This is an illustration of a general law in physics, namely, that bodies which can emit a wave of a certain kind are also able to absorb a wave of precisely the same sort. You see the two flames here which are separated by a considerable interval; one of them is burning quite still, and it might remain in this quiescent state for any length of time. I place a tube over the second one so as to make it sing, and after a short time you will hear not one note but two. Now you hear two notes in unison. The second one has been started as it were by the action of the first.

Again, by lowering the tube upon the first I cause it to give a different note, and you perceive that the second one does not now respond to it; it is quite dumb. A flame will respond to a note of its own wave length, but it will not respond to a note of a different wave length. This is a phenomenon of exactly the same kind as that a body will absorb heat rays of the same sort that it will itself give out when heated. Or again, a piano wire, when stretched, can emit a particular note, and if we sound that same note in its immediate neighbourhood the stretched wire of the piano will respond to it, and will give out the same note. Again, many of you know that when a glass partly filled with water is rubbed by a wetted finger, it gives out a certain note. If I strike this note strongly on a piano, or on the string of a violin, I can make the glass respond to it. These are a few examples of bodies absorbing that sort of wave which they are able themselves to give out.

Some of you may perhaps have observed that when a gas flame is on the point of flaring it is in a particularly sensitive

condition, and noises made near it seem to affect it; and often the flame, instead of burning quietly, will throw out long tongues of fire. Workmen have frequently noticed, for example, in manufactories, that a creaking noise made by their tools at certain times in the evening, when the gas is at a considerable pressure, will exercise a remarkable influence upon the gas, which is seen to jump up and down when these noises are produced. Musicians also, at concerts, have noticed the same thing—that when the gas is on the point of flaring, certain notes made the gas respond, as it were, to those notes. I have got here an ordinary fish-tail burner, and the pressure is so regulated that it is in this sensitive condition. You see that a little squeaking noise makes it respond at once. [Experiments.] And even if the noise be made at a considerable distance, say at the end of the room, the same result will follow, and the flame will respond to this peculiar noise. I shall now take a bat's-wing burner. You see this also responds, although not so much as in the last example, still it evidently responds to every noise that is made. I shall now take a much more sensitive flame. I shall take a jet which issues from a glass tube drawn out to a fine point. We have got a long jet which burns with a sort of excrescence at the top. Let us so regulate the pressure as to get this just on the point of flaring, but not actually flaring. There is now a flame which you will find to be particularly sensitive to different sounds. [Illustrations of an amusing nature, in which the audience took part.] You find that the flame is not only sensitive to this squeaking noise, but to many different sounds. If I make a noise by clapping my hands, you see it responds. [The audience clapped.] The flame responds energetically to the noise which you make. It is not the puffs of air which you generate by clapping hands that affects the flame; it is the waves caused by the noise itself, for when we puff upon this flame with a bellows, gently, not directly upon the flame but a little to one side, the silent puffs of air do not affect it as the noise does. These singing flames will respond not only to squeaking noises and the clapping of hands, but to the sounds of certain letters—if I hiss, for example, at it. [The lecturer sounded the letter *s* strongly.] It is very sensitive to the sound of the letter *s*. [The flame jumped when the letter *r* was trilled, and danced again when the audience hissed.] It is by regulating the width of the orifice of the gas and the pressure that we get the flames that are sensitive to different sounds. Generally the higher the pressure is the higher also is the note to which the gas will respond. I might strike certain notes on the piano, but

to certain of them it would be quite dumb, whilst to others it would violently respond.

Now, what is the explanation of this somewhat singular phenomenon? Why is it that flame should be in this critical condition, so as to respond to very slight sounds indeed? In fact, our flame seems to resemble one of those large rocking stones that you may possibly have read of as occurring in many parts of England, especially in Cornwall. These large masses of stone are sometimes so delicately poised that the slightest touch of the hand will make them rock; hence they are called logging or rocking stones. Now, our sensitive flame here seems to be in this critical condition. It is, as it were, on the edge of a precipice, so that a slight force serves to push it over. But how is it that a noise of a certain sort will make it respond? The shrinking of a tall flame into one of less than half its height is equally produced when the pressure on the gas is slightly increased, so that the effect is due to a change of pressure. The pressure with which the gas emerges will depend, in the first place, upon the weight placed on the gas-bag, and, in the second place, upon the width of our orifice.

Now, if while the gas is issuing the orifice be contracted, it is quite clear that the pressure is virtually increased. And certain sounds can throw the tubes through which the gas is issuing into vibration—namely, those sounds which the tubes can themselves emit. If, therefore, I sound such a note that our glass tube vibrates in response to it, then we have the orifice alternately contracted and expanded; and, therefore, the pressure of the gas will be correspondingly altered. It is in consequence, therefore, of this varying pressure caused by the alternate contraction and expansion of the orifice that the sudden shrinking of the flame is produced.

I shall now show you, before bringing this lecture to a close, an experiment which I dare say many of you have seen before; but I wish to draw a different lesson from it to what was derived when you saw the experiment performed in Dr. Roscoe's last lecture. I have here a large Bunsen gas lamp of the ordinary construction. I set fire to the gas, and of course it burns with the ordinary non-luminous flame. Now I shall render this luminous by introducing into it a little carbonate of soda. If you now look at any coloured objects in the room, you will see that instead of being of their usual tint they are of a ghastly yellow hue; and if you examine your own and your neighbour's hands and faces you will see the same corpse-like tint. I wish you also to notice that while colours generally appear of this ghastly yellow, any black object still

appears black. I shall repeat the experiment, and I wish you to look at your black coats. You will find that they are still black, while all other colours than black are changed. Now I wish to derive from this simple experiment some information as to the natural colours of bodies. How is it, for example, that some bodies appear red, others blue, others orange? In other words, to what is it that bodies owe their natural colour? This experiment shows us that it is not something inherent in the body itself, because if a piece of red cloth were inherently red, it ought to be red in whatever light we examine it. Clearly, therefore, the red colour is not inherent in the object itself; it somehow or other owes its colour to the light which falls upon it. Now you have in previous lectures been shown that white light consists of a great many different colours; that you have, for example, red, orange, yellow, green, blue, indigo, and violet, mixed in white light. Now when this white light falls upon bodies, some of it will be reflected and part of it will be absorbed; and each different object, of course, will absorb its own definite colour or set of colours, and will reflect to our eye the remainder. It is because the piece of red cloth, for example, reflects the red rays and absorbs all the others, that it appears to us to be red. Again, a piece of black cloth appears black because it absorbs all the colours and reflects none. But I should guard this expression, and say not that a piece of red cloth, for example, reflects only red rays, but, rather, that it reflects the red rays in greater abundance than the others. It does reflect a few others; it reflects, for example, a little orange and yellow, but no blue. Similarly a blue object reflects principally blue rays, but it will also reflect a few yellow ones, but hardly any red ones. So that if we change the character of the light which falls upon objects, we of course change their colours. Now the light which you saw last differs from white light in being a perfectly pure light; that is, it is not a mixture of light of different colours, but consists only of yellow rays. So that if an object is not able to reflect any yellow rays, then when such an object is illuminated by this pure light it will appear, of course, to be black, simply because it is not able to reflect any rays at all. This will explain how it is that your black coats still appear black. A red object does not appear black in this light, as it would do if it reflected only red rays, but as it is able to reflect a few yellow rays it appears of a pale yellow colour. Thus all coloured objects will change their colour according to the nature of the light by which they are illuminated, and in the absence of all light, that is in darkness, are of course black.

THE LIFE OF FARADAY.

A LECTURE

BY DR. J H. GLADSTONE, F.R.S.

Delivered in the Memorial Hall, Manchester, Dec. 4, 1872.

THERE are great men who are proud of their descent from a noble ancestry; there are other great men who are proud of their ascent from an unknown parentage, and take pleasure in talking of how they have become the architects of their own fortunes, and how their present position has been achieved by their own strength of arm or strength of will. If Faraday had been proud of his origin it must have been in the latter sense, for he was the son of a journeyman blacksmith, who worked in London towards the close of the last century. He was one of a very numerous family, and was born on September 22, 1791. Yet no man is, or can be, independent of his parentage, and Michael Faraday was no exception. His father, I have said, was a blacksmith; but I have, within this last week or two, learned that he was a very clever workman, and long after his death, if his fellow-workmen came upon a piece of iron that was particularly well shaped from the fire, and required little or no dressing with the file, they said it was "Faraday's work." Again, his father was a very industrious man, although he was afflicted with bad health, which brought him to a premature grave. As for his mother, she was a grand woman, they all say, and she was "particularly neat and nice in her household arrangements." We shall see presently that this clever manipulation of the father, and this neatness and orderliness of the mother, were reproduced to an extraordinary degree in the son. And, in other respects, theirs was no common household. For two or three generations, at least, the Faradays

had been an industrious, moral, religious family. They belonged to a peculiar sect of Christians termed the Sandemanians, a sect little known, and with little to distinguish them from many others; but they have kept very much to themselves, and do so still, and this, continued for more than a century, causes in that community a certain separation from what is common or worldly round about them, and produces a peculiar kind of refinement and home affection. It was, therefore, in this atmosphere of poverty, but, at the same time, in an atmosphere of high moral worth, that Michael Faraday was brought up. And he often looked back upon the days of his childhood, and seems to have had no common affection for his home. This often peeped out in after days. For instance, when Noble, the sculptor, was finishing that bust [here the lecturer pointed to a large photograph of the bust of Faraday by Noble], he happened to rattle his chisels together, and, noticing that his sitter had become vacant in look, he said, "I am afraid that the noise disturbs you, or that you are weary." "No, my dear Mr. Noble," said Faraday, and he put his hand upon the sculptor's shoulder—"no, but the jingling of your tools took me back to my father's anvil, and I was among old scenes."

We must, therefore, imagine him as a boy brought up in this industrious home, where, although there was no doubt a severe struggle for the means of life, owing to the increasing number of children and the ill-health of the head of the house, yet, though poverty came in at the door, love did not fly out of the window. We must imagine him to be receiving just the smallest amount of education, first of all at a dame school, and afterwards at something rather better than that. But the instruction which he received at that time was simply the foundation of that education which was growing in all his after life; for, as you and I know, what we learn at school is but a very small part of that which we learn altogether, if we are thinking men. And so it was with Faraday; for as we trace his life we shall find that he constantly went on improving his mind from all the circumstances that were round about him.

We know little of his childhood, but some stories that we have show that he was even at that time something of an experimenter and inquirer. I will give you two of the only stories I know. One is this. He was at his father's workshop, in the upper storey, where there was a large open space just over the anvil or forge; and little Faraday was there amusing himself with pitching halfpence into a pint pot, and when he found he could succeed very well at a certain distance he began to try his ability

at a greater distance, and, stepping backwards, he fell down this hole and nearly killed himself; but, fortunately, he fell upon his father's back, which saved his. Here, then, we find the young philosopher at work trying experiments, as he experimented all his life long; and not content with his first successes, he must try if he could not improve upon them.

Here is another story. He was carrying out some papers to a house, and while waiting at the door to be answered by the servant, he put his head through the iron railings. And then he began to debate within himself this difficult problem—which side of the railings he was on. That was rather a metaphysical inquiry, but it shows the thinking boy at any rate, though like many such questions it was never resolved, because the servant opened the door and he pulled himself back in a hurry, and hurt himself so that he remembered it in after days.

Well, so he went on till he was thirteen years of age, and then he must do something for his living. He went first of all as an errand boy to a book shop; and he behaved with such industry, perseverance, and propriety there for twelve months, that when he was fourteen, his master, Mr. Riebau, took him as an apprentice without a premium, and he remained at this book shop for seven years. During that time he learned the art of bookbinding. During the latter part of his apprenticeship also he was frequently reading the books which came into his hands; and at the same time he took opportunities to hear lectures. Penny lectures there were not I think in those days, but he got his elder brother, Robert, to give him shillings, so that he went to hear some lectures on science at a shilling, and he wrote them out afterwards. I have looked over the notes which he took of these lectures in those very early days; they are very interesting, and are now among the treasures of the Royal Institution in London. He learned, too, to draw from a French artist of eminence, who happened to be lodging at his master's house. He got acquainted with a good number of young men of like tastes with himself, and they carried on a mutual improvement society, which afterwards was called the City Philosophical Society, and developed in various ways, until it formed to a certain extent the nucleus of the present Society of Arts. But the young man's mind was set upon a more scientific employment than that of selling or of binding books. At that time Sir Humphrey Davy was a man of great eminence in the scientific world; his discoveries resounded throughout Europe, and his lectures were the resort of the most fashionable audiences. Young Faraday was entranced with the

fame of Davy; and having read a good deal that Davy had written, he got the opportunity of hearing the last four lectures which that philosopher ever delivered at the Royal Institution in London. He wrote these lectures out very carefully, making little drawings of the apparatus; and circumstances led him some months afterwards to send this book to Davy, with a letter explaining his desire for some scientific employment. This was well received by the great philosopher of Albemarle Street, and eventually Davy employed the young aspirant as his assistant in the laboratory, at 25s. a week, with two rooms at the top of the house. This was on the 1st March, 1813. Here the young Michael Faraday found himself in a congenial occupation. His work was hard, no doubt, and among explosions and noxious gases he ran some risk probably in Davy's laboratory; but still he worked on with an honest intention and with a cheerful heart, solacing himself occasionally with a song, or with playing upon his flute, and found very little time to write to some of his old friends, with whom he had corresponded largely before. But shortly afterwards there was an event which did very much for Faraday's education—that was his going on the continent. 1813, as you may remember, was a time of much political disturbance in Europe; but the philosopher Davy, with his wife, and young Faraday as his secretary, travelled about in various places, and visited the most eminent men of science upon the continent. Faraday's mind was, of course, enlarged greatly during this process. In those days, you know, postal communication was rather difficult, especially when there was war in various parts of Europe. Here is a letter which Faraday wrote at that time from Rome. It cost 3s. 10d., I see by the postmark. It is dated "Rome, Dec. 21, 1814," but was mostly written the day before Christmas Day. It is addressed to his eldest sister, and is full of all kinds of familiar chit-chat, and inquiries about the babies—there had been a fresh baby in the establishment. There is also an allusion to his eldest brother having got engaged to be married, and various other little matters of a private character, with descriptions of his first impressions of Rome, and the Carnival that was to be, and so on, along with occasionally a warm-hearted expression such as this—just before Christmas Day: "Hail to the season! may it bring every blessing down upon you. May it fill your hearts with gladness and your minds with contentment. May it come smiling as the morn which ushers in the glorious light of a summer's day; and may it never return to see you in sorrow or trouble.

My heart expands to the idea that Christmas is come, for I know that my friends, in the midst of their pleasures, will think of me," &c.

He came back to England, and was engaged again in the laboratory of the Royal Institution, where he worked and eventually lectured, for gradually the assistant became the lecturer. Professor Brande was the Professor of Chemistry at that time, and a very good lecturer he was, but Faraday occasionally took his place. I was talking lately with a lady who in those days was a girl, and she told me that she remembered being taken along with her sister to hear the lectures of Brande; and when they went into the room she said they were rather apt to inquire whether Brande or Faraday was to lecture that day, because they decidedly preferred the lively young assistant to the staid Professor. In this industrious, intelligent way, he went on for some years making inquiries of nature, and constantly ascertaining fresh and fresh facts—of which more hereafter,—and then came some changes in his life.

We arrive at the period of 1821, when Faraday's position was rather improved at the Royal Institution. He was acquainted with a goldsmith in Paternoster Row who had some daughters, and his visits to that house became more and more frequent; at last he wrote a beautiful letter, that has been preserved, to one of these daughters, asking her if she could not be more than a friend to him, but hoping that at any rate he might retain the friendship which he had, saying that she had corrected him of some bad habits, and he hoped she would correct him of more, and so on. The lady hesitated, and went to Margate; but he followed her there; and then they went together to Dover and to Shakespere's Cliff, and he returned a supremely happy man. Shortly afterwards they were married, and thus commenced one of the most beautiful domestic lives of which we have a record. They lived together united in an affection which we have heard described by one of her relations as romantic. They never had any children; but Faraday was very fond of children, and he generally had some of his nieces staying with him; in fact, three of them were in a great measure brought up by him. There was another important event also which occurred in the same year. His parents being Christian people, there are indications even in his early letters that his mind was directed sometimes to religious things. We have evidence that he thought seriously over these matters about this time, and he publicly professed his faith in Christ by joining the church of his fathers shortly after his marriage. And he was

constantly attached to it, joining most regularly in its services, and afterwards becoming one of its elders and preachers. This quiet life continued long—he living at the Royal Institution, working in the laboratory, making fresh discoveries, and frequently giving lectures.

We may pass over another ten years, and come to 1831. There was now the great question before him—what was to be the object of his life? At that time he was making a very considerable income by commercial or analytical chemistry, and he was much sought after in various directions. The question came—to what should he devote himself? Just then he was making some of his most important discoveries respecting the connection between magnetism and other forms of electricity. At last he determined that his life should be devoted to these matters of research, and that he would be an experimental inquirer into scientific truth, and a revealer of that truth to others. For this purpose he gave up the pursuit of wealth. His receipts for commercial analyses had latterly been about £1,000 a year; they dropped down to little more than £100 in that year, and afterwards he scarcely made anything at all in that way. He gave up also the pursuit of position and renown in society. But we cannot say that money and honour gave him up. Although he did not pursue wealth, still we must not suppose, as has frequently been said, that he was a poor man, for there were several appointments which he held, and which brought him in always a fair income, and the Government gave him £300 a year. Then as to honours, all sorts of scientific honours were showered down upon him by nearly all the learned societies in Europe; but he did not care for those that brought with them serious obligations, and he declined some of the highest that he was capable of receiving; for instance, the Presidentship of the Royal Society; and when once he was sounded as to whether a knighthood would be acceptable to him, he declined the proffered honour, saying that he must "remain plain Michael Faraday to the last." For the pursuit of this one great purpose in life—that of an experimental inquirer into truth, he gave up therefore a great deal that men esteem most precious in life. He also did not intermix himself with many things which we cannot but suppose he was interested in, such as the great social questions of the day. It is probably difficult for us to understand why Faraday, with his large and loving sympathy with men, did not engage more, or rather did not engage at all, in any ot the philanthropic movements of the time. Every man must ot course be judged according to his own conscience in this respect; but one could almost have wished that

he had not so exclusively kept himself apart, but that he had given some of his time, or at any rate the strength of his name, to some of these movements for the improvement of the condition of his fellow men. However, he kept himself to his laboratory, his home, and his church.

Faraday had indeed a passion for experimenting. Without going so far back as to refer again to his childhood, we might speak of the simple experiments he made at the end of his friend Abbott's kitchen table before ever he was employed by Davy. Faraday's first original research was published in 1816: it was merely an analysis of some caustic lime; but that was the first of an extensive and beautiful series of investigations, so numerous that the Royal Society's catalogue contains the titles of 158 papers. A good experiment seems to have filled him with the most intense pleasure; but he always liked to have his own fingers in the experiment. That manipulative skill to which I have referred was manifest throughout all his life, and the order and neatness of all his arrangements were remarkable. If you had gone to see him in his laboratory, you would have found him wearing a large apron, with just the apparatus he wanted, and no more, arranged before him on the table, and this apparatus generally of the simplest kind that would serve his purpose; and then he would begin with those wonderful broad-ended fingers of his, twisting about wires, blowing glass, or working skilfully with corks and cards, and other things, to produce the contrivances that he wanted, and all the time balancing himself on one foot and then on the other, and swinging himself from side to side while he observed what took place; and then if nature gave him an affirmative reply to his questions, his face would brighten with pleasure; but if there came a negative reply, still that was information. Sometimes there would be a doubt as to what the nature of the result actually was, and then he would modify his experiment, so as to arrive at the truth. Everything must be neat and orderly; every bottle must have its stopper in, and every basin or glass, if put away, must have a nice paper cover put upon it; and there must be no dust or dirt about that could possibly be avoided. In this way he was constantly working; and no small part of his time was passed in inventing and perfecting his apparatus. He started generally with very simple things. I will show you a little of his apparatus. Here is a box which Faraday took about with him in his lighthouse expeditions, and which was kindly lent me for these lectures by the Trinity House of London. I will take out of that box this little one. Sir Frederick Arrow, the deputy master of the

Trinity House, told me a story about this small box, relating to a period of Faraday's life when he went round our coast to make observations and comparisons of the different lighthouses. The persons composing the expedition took all the photometers they had for the purposes of observation. Producing this little card box, Faraday said, humorously, "I must take particular care of this, because it contains my special photometer;" and when they were all prepared for making their experiments he produced his special photometer—and there it is. You see that it is merely a little shawl-pin with a black bead of glass at the head; and an admirable photometer it is, because, instead of attempting to compare the different lights by looking at them separately, you have only to get them reflected upon that bead, and you are at once able to compare the brilliancy of the lights. Here, then, is Faraday's "special photometer." But he did not stop with the first idea. Here is a whole heap of curious contrivances of the kind; little silvered glass beads stuck upon corks, stands upon which they were to be placed, made of wood and steadied with lead. These things are all, I believe, intended for improvements upon the original shawl-pin. The wooden box which contains the whole was originally, you see, a cigar-box, and one of the card boxes inside, now filled with glass apparatus, bears still the label "Improved Seidlitz Powders." Here are some of Faraday's standard candles; and here is his candlestick in three different stages of development. There is first the simplest idea of a candlestick—only a round flat piece of lead; you see you can stick the candle upon it, and there it is. Only that is a little awkward to carry. The next form is this; it is much the same, only it has a little handle which you can catch hold of—that is more advanced. But *the* candlestick is this, and it is, I believe, of his own making—a piece of solder poured out upon a plate, in somewhat the shape of a pear. You can stand the candle upon that, and I believe if you heat it the candle will stick very well. It is turned up at one end so as to form a thumb rest; and what better candlestick can you possibly have than that? At any rate, that was the candlestick which was made and used by the greatest philosopher of the day. These things are of the more interest to me because I was associated with him at one time in these examinations of lighthouses, for I happened to be appointed a member of the Royal Commission which reported upon our lighthouse system, and as Faraday was scientific adviser to the Trinity House, we worked together upon this matter. This is one of Faraday's drawings of a lighthouse flame. There is the wick, and here is

the flame rising from it. I have not time to enter into a description of the beautiful optical apparatus which is put round the lighthouse lamp. We generally found the lamp and the lenses and prisms badly adjusted; or rather there was no proper means of adjustment. But we devised a means, and brought it under the attention of Faraday, and Faraday elaborated it, and contrived a little apparatus, of which he made this drawing for the instrument-maker. You will see by that how well he could draw out a plan for a piece of apparatus, for the instrument maker, Mr. Ladd, had no occasion to vary from the drawing, excepting in some minute proportions. At the same time he supplied a model of the apparatus made from a cork, cut into the proper shape, with two lucifer matches stuck through it. I cannot enter into a full description of the whole matter, but I just show you the apparatus because I want you to see the simple way in which he worked out his ideas and planned his apparatus, making drawings and rough models for the instrument-maker to work from. That apparatus, I believe, has been used for examining the lights all round the coasts of this kingdom, and has thus aided in improving the illumination of our shores, and securing the safety of our mariners.

I have brought with me this other piece of apparatus which Mr. Ladd has lent me, and which is connected with the same inquiry, though it was never used. It affords the means of making a brilliant spark in any part of the space usually occupied by the flame of a lighthouse lamp, and the direction of the rays from that particular point can be ascertained from outside the apparatus. The main interest connected with this instrument arises from the fact that it is about the last piece of apparatus that Faraday ever devised.

This power of adapting simple means to produce the end required was exhibited by him in various other ways, and even in the common concerns of life. For instance, on one occasion a flower was given to him when he was away from home, and which he wished to take with him without its fading. I will show you how he managed this: Here is a flower which I brought from Professor Roscoe's dinner table just now. Well, Faraday first took a piece of ordinary letter paper, rolled it round the cork of a medicine phial, thus, and tied it with a string. There you have a little tube, into which you may pour water and stick your flower. There is Faraday's bouquet-holder, and what more perfect bouquet-holder can you have? Any of you may make one like it, and it will hold water for many hours.

Faraday liked to show the boys and girls to whom he lectured at Christmas time, in the Royal Institution, in what a simple way he could produce his experiments. I must show you one of his contrivances—his electrophorus. You know the piece of apparatus called an electrophorus. It consists of a plate of resinous matter, which is rubbed with a catskin to excite electricity, and a brass plate, to which is attached a glass handle, so that it can be laid on the resinous plate, and removed from it when charged, without discharging itself through the experimenter's hands. Such an apparatus you can buy at an instrument-maker's; but Faraday wished to show that he could produce an electrophorus without going to the instrument-maker. He liked to show boys and girls that these effects could be produced by means which they could find at their fathers' houses, or could buy at the cost of a few pence. Here is a piece of indiarubber cloth, and a piece of tinfoil; it is a rough piece I found in my laboratory, but that does not matter much; at any rate we must take what means we can get hold of without going far to look for them. I will press it down to make it more flat; and now I put a plate or saucer upon it. The object is to lift up the tinfoil. I take some strings of silk, and make a sort of cradle. I first put the tinfoil on the table, and the silk cradle on top of it; then I put down the plate upside down, and I can turn up the ends of the tinfoil over the plate, so that it will hold the sheet of metal. Now we want to excite the piece of indiarubber. How shall it be done? I can do it by simply beating it with a piece of flannel, which is more easily got than a catskin. I have not taken any precautions to warm this, as lecturers usually do with their electrical apparatus. Now, by means of the silk threads I will lift up this tinfoil and plate and put it on the indiarubber, and then touching it for an instant with my finger, and raising it again, you perceive I can get a considerable spark on my knuckle. Those of you who are near will plainly see or hear the spark. That is the simple way in which Faraday made an electrophorus.

But this passion for experimenting and this manipulative skill would never have made him the great philosopher he was. If he had worked constantly, of course he must have made some discoveries; but if a man merely makes apparatus, sets that apparatus working, and watches what happens, without any particular view, nature is not likely to give him very satisfactory or very instructive answers. If he carries out more fully, as some do, the experiments of others, amassing new data, for instance, that is a most valuable service, but it is the work rather of an apprentice than of a master.

Faraday did not do that. He usually had some idea or conception in his mind, and then the experiments were devised so as to prove the truth or falsity of that idea. There were about him two valuable qualities from the very beginning. He says, speaking of his own early life: "I was a very lively, imaginative person, and could believe in the 'Arabian Nights' as easily as in the 'Encyclopædia'; but facts were important to me, and saved me. I could trust a fact, but always cross-examined an assertion." And in later life he was greatly indebted to his wonderful imagination, and at the same time his great love of truth, his loyalty to truth; so that while his imagination carried him along and opened up fresh vistas of thought and experiment, his love of truth prevented him from being carried away or misled by this power of imagination, while always obeying the indications of nature; and those who have worked upon the confines of human knowledge know how very difficult indeed it is not to be misled by preconceived ideas.

We must bear in mind, also, that with this great imagination and loyalty to truth, Faraday had the advantage, for it was an advantage to him,—I believe—of being free from any system to begin with; he was ready to follow Truth wherever she led.

And now what can I say as to the results of his scientific work? Through the kindness of Mr. Harrison, I am able to show you just a few experiments that may illustrate here and there something of his discoveries; but I must refer you to the book of Tyndall on Faraday as a Discoverer for any real account of his labours. The work of a lifetime cannot be condensed, of course, into one lecture, much less into a small fraction of a lecture. Faraday's experiments extended through many different regions, and to understand and appreciate his labours fairly it would be necessary to go back into the position of science at the time in which he commenced, and that would be difficult for any of us to do. It was as a chemist that he began. His discoveries of chemical substances, however, were not numerous, or perhaps generally important; but here is one—benzine—which has acquired considerable importance since. You know that benzine is used in many of the arts. One of the children of benzine also is aniline, and the children of aniline, and therefore the grandchildren of benzine, are many beautiful colouring matters, which I need not put upon the table, for some of my lady auditors, I dare say, have brought specimens with them—the mauve and magenta dyes: all that branch of industry has sprung from Faraday's discovery of benzine. At the time that he began experimenting

it was supposed that gases were very different things from vapours; but he showed that it was merely a question of temperature whether any body, not decomposable by heat, should exist as a solid, a liquid, or a gas. Then, again, he showed the connection of chemistry with galvanism; that the chemical action in the battery was the measure of the galvanic electricity that was produced. But the greatest series of Faraday's researches were those which showed the connection between one form of electricity and another--that the same great force was capable of manifesting itself under various aspects. One or two illustrations I may give you. There is a battery below the table, and the chemical action is going on unseen, and the power is being carried through these wires, and now it is proceeding forth, not as chemical action, but as light. You see the intensity of that light between the charcoal points. [Illustration of the electrical light.] Here we have the transformation of one force into another. Of course, I need not say that along with the light there is a great deal of heat. But we will take something which was more especially Faraday's. The next experiment is to illustrate his induction coil. I had hoped that the lecture preceding mine would have been by my friend Mr. Barrett, who has been prevented, as you are aware, by illness, from giving an account of Faraday's electrical discoveries. If Mr. Barrett had lectured, I should have drawn upon your knowledge of his lecture for these illustrations. But we will have one or two. Again we start with the chemical power, again we have galvanic action, and now we find the production of magnetism, and induced currents of electricity, giving rise to the lightning flash between these two points. You hear the loud report and see the light. The flash is much the same as that produced from an ordinary electrical machine, where it originates in friction. We shall now pass it through some vacuum tubes, and you will see other luminous appearances. I may repeat that the same power is here presenting itself in different parts of the apparatus in different forms. Commencing with the solution of a metal in an acid, we get finally this beautiful hydrogen light. It is only near at hand that we can see the extreme beauty of the forms, and the bending of the light in various directions, and its division into luminous strata. Here we have a large tube of the same kind, with a fluorescent solution, and here one with cups of uranium glass. [These and other experiments were ably performed by Mr. Harrison.] One of Faraday's greatest discoveries, and a very prolific one it was, consisted in making a magnet rotate, by which he was able to produce any electrical effect,

including the spark which was afterwards exalted into the brightest of all lights. Faraday also showed the connection of magnetism with light by causing it to rotate the plane of a polarised ray. It seems a pity we have not time for more of these beautiful illustrations which Mr. Harrison has kindly prepared and brought before us, but we ought to have this little apparatus in which electricity is set to do mechanical work. Here is a small pump, and the water is to be pumped up by means of these magnets. The power is brought from the galvanic battery below, and we have an arrangement for converting this chemical and electrical force into a simple mechanical force. There you see the stream of water raised by the electric pump.

Faraday's work consisted in a great measure of the overthrowing of idols. There were many false opinions prevalent about the forces of nature at the time when he started his investigations, and he did much good service in overthrowing them, and trying to get rid of the false notions that attached themselves to such words as "poles" and "currents." We cannot say that he always got rid of erroneous opinions himself, because so difficult is it to separate our notions from the words we employ, so difficult is it to clarify our intellects, that no man—Faraday or any other—can become altogether independent of the tyrannical influence which words exercise over his ideas. A still more important service which Faraday rendered was in the breaking down of barriers. When he started his investigations all the different sciences seemed to stand aloof from one another; but he gradually showed the connection of one with the other, and exemplified how force was capable of appearing sometimes in one form and sometimes in another — while the same amount of force was continually present. Other philosophers, including our chairman, have rendered inestimable service in carrying out more fully the same ideas, with mathematical knowledge that Faraday did not possess. These barriers between the different sciences Faraday broke down, and showed that the whole of nature was a commonwealth, and he tried to demonstrate that there was one great law pervading the whole. And not only did he break down the barriers that had been set up by men, but he enlarged the boundaries of science, and added a great deal to the wealth of our previous knowledge, making known to us a great number of new facts, which others have taken as starting points to make additional discoveries. Not only did he thus overthrow idols, break down barriers, and enlarge the boundaries of natural knowledge, but he often raised an enthusiasm for science. It is

true that he had no disciples, properly speaking, that he had no body of young men whom he trained in the paths of research. That did not seem to be within his power; but he lectured as very few indeed have ever lectured, and he imparted his own enthusiasm to the audiences that hung upon his lips. At the same time he raised the popular conception of science. In his early days he wrote thus :—

"A philosopher should be a man willing to listen to every suggestion, but determined to judge for himself He should not be biassed by appearances, have no favourite hypothesis, be of no school, and in doctrine have no master. He should not be a respecter of persons, but of things. Truth should be his primary object. If to these qualities be added industry, he may, indeed, hope to walk within the veil of the temple of nature."

And what he laid down then as the characteristic of a philosopher, he carried out in his future life and taught to others. Faraday also did good service in promoting scientific education, for so impressed was he with the importance of our knowing about the various forces which are constantly influencing us throughout this beautiful Nature which is spread around us—the handiwork of God—that he claimed for the study of nature a place in the education of all men. I do not know that I need speak so much about that here in Manchester as in some other places; but without in any way depreciating other kinds of study which are perfectly necessary, I think we may well say that a man whose knowledge of the world around him, whose knowledge of nature, has not been properly drawn out and trained, is only a partially educated man. And so, ladies and gentlemen, I would claim in Faraday's name—as I think he would if he were standing here—an honoured place for science in the education of every Englishman and every Englishwoman.

Now will you go back with me again to Faraday's personal character for a little? We have seen something of the great work that he achieved, the mighty results that sprang from his diligent, constant labours down in the laboratory in the basement of the Royal Institution in London; but in the evening he would go into the upper part of the house and be with his wife and nieces, or he would be enjoying himself with various other friends in a social way, inviting them to supper—nice pleasant little suppers they were, nothing extravagant or pretentious about them, but everything good, for Faraday was no ascetic; and what was far better than the things on the table was the geniality that was round about the table. Then there was the kindliness that

pervaded all his intercourse with others, and his love for children too. I remember how he played with my own children. One of his nieces gave me this the other day; it is a copy of a letter he sent to her daughter, then a little damsel five years of age. I think I may read the whole of it; it is short and sweet, as well as characteristic:—

TO CONSTANCE DEACON.

Royal Institution, 19th May, 1852.

My dear Constance,—First a kiss, p— p— p— ph; next thank you for your good letter—very well written, and very pleasant; and now thanks for the letter you are going to write to me, in which you must tell me how Papa and Mamma do, and what you are about.

I went this morning to see a fish like a great eel take his breakfast. This morning he had three frogs for breakfast; yesterday he ate nine fish in the course of the day, each as large as a sprat, and the day before fourteen. When the fish are put into the water he electrifies and kills them, and then swallows them up, and if a man happens to have his hands in the water at the same time, the fish—that is, the eel—electrifies the man too. The eel is now above twelve years old, and is heavier, I think, than you are.

Yesterday I saw the Royal children, the Prince of Wales, and the Duke of York—such nice children! they would make famous playmates for you; but I do not know whether princes do play much. I do not think they can be as happy in their play as you are.

As to the magnic*, when you and I meet we will have a long talk about it, and make some *experiments*. And so, with my love to Papa and Mamma, and curious Constance, with a kiss for each, I am your loving old uncle,

M. FARADAY.

You may form a fair idea of the appearance and expression of the good man's face by looking at these different portraits, and combining them together in your mind. [Here the lecturer showed the portraits of Faraday in the memoirs written by Dr. Bence Jones and Prof. Tyndall, and some other engravings and photographs, especially commending one by Watkins.]

We may imagine the genial philosopher also at his various amusements, for he was fond of seeing all the sights of London, all the shilling shows, the Zoological Gardens, or the opera; and his enjoyment was the greater if his wife or niece was with him. Then there was a remarkable playfulness which pervaded his whole nature, and relieved his work, a playfulness which it is difficult to describe, because it depended mainly upon his manner; but a playfulness sometimes rising into a little practical joke of a harmless nature, though generally stopping short of that, and diffusing a kind of pleasant halo over one's intercourse with him. And under his playfulness there were deeper feelings.

* I believe this was the little girl's way of pronouncing "magnet."

There was a great reverence which seemed to characterise the man—a reverence for everything that was about him that deserved respect; a reverence not for God only, not for nature only, but for man, for all men, unless he knew them to be bad. This reverence appeared also in the form of self-respect. It was difficult to take any liberties with him. It was strange, too, how, in talking with him, you felt that he was giving you credit for high motives and for noble aims, while perhaps you felt ashamed of yourself, being aware that your own aims were not as high as those which Faraday was imputing to you. And so the general effect of intercourse with him—at any rate I felt it to be so—was that of a moral tonic, and one went away from the good man feeling braced and stronger for the various duties of life.

And, in addition to this, there was a peculiar amiability in his character—a gentleness combined with firmness, an unselfish kindliness in all his relations with others. He loved his friends with a rare love; but he was ready to do acts of service to strangers. [Here the lecturer repeated some of the illustrative anecdotes that are printed in his book on "Michael Faraday," and elsewhere.] This kindliness showed itself unconsciously in a thousand little loving actions. It caused him to give away large sums of money, and to spend time in visiting the sick, without taking note of the outlay, and it won the hearts and the confidence of all who came into his company.

Yet when I speak of Faraday's moral excellence, you must not suppose that he was always perfect, or had no need of that self-control which every true man must exercise. The beauty of his character does not lie in its being faultless, but we should rather admire him for having learnt to curb those risings of pride or irritability which might otherwise have marred his nobility and unselfishness.

We must think of him, also, not only on the ordinary working days of life, but also on Sundays. I will not enter into the peculiarities of the small religious body to which he belonged, but simply mention, that, as an elder, he took his turn in preaching among them. His sermons, however, were not to be compared to his lectures. One could, perhaps, have wished that he had thought it right to apply to his interpretation of the word of God some of those great principles that had been so productive in his own hands in the investigation of natural science. But he did not do so, and his preaching was of the simplest and plainest order—chiefly a collection of texts illustrating some central thought, such as the following, which I quote from one of his sermons: "The plan

of salvation is so simple that every one can understand it—love to Christ springing from the love He bears us, and which made Him undertake our salvation."

But I must hasten on to the close of his life. His was a gradual decline. I have not time to read letters in which he speaks of how happy his life had been. And yet with all that, he was not loth to depart, but said, if asked how he was—"I am as well as I expect or wish to be, for I cannot expect or desire to be here any longer, as now I am only a burden to my friends." And gradually old age and the failing of his faculties crept over him. He retired more and more to the house which the Queen had given him near Hampton Court, and there he mainly spent the last few years of his life. His niece and constant kind attendant, Miss Barnard, told me lately an affecting story, how one day in 1864 he brought her a copy of the *Athenæum*, in which was narrated an incident of the great Duke of Marlborough wishing toward the end of his career never to be consulted upon any important matter of state again, because he felt that his powers were failing. Faraday, with tears in his eyes, wished her to copy out the passage, and remind him of it if the need arose. Some time afterwards, when he was very feeble, he proposed to her a journey to Paris. On his persisting in the idea she asked if he remembered the story of the Duke of Marlborough. At his request she fetched the paper, and read it to him. He thanked her, and said nothing more about the Paris visit. Gradually decay came on; his memory and other faculties failed; silently and slowly he sank to rest; and on August 25, 1867, this beautiful spirit passed beyond our earthly horizon.

But although his bodily presence is gone he lives still amongst us by his works, ever fruitful in fresh applications. He lives also in the enthusiasm which he has engendered in the hearts of very many for natural science. He lives, moreover, by the example which he showed of the combination of intellectual and moral greatness. In him could be seen a living instance of how a man may unite nobility and strength of character with perfect gentleness and love; how he may cherish a beautiful domestic life, and yet at the same time be one of the princes of intellect; and how this lovable, simple-minded, honest, upright godly life may be one which the present age and succeeding ages will delight most to honour. When Faraday passed away the world mourned him, and it still mourns him, because it has lost not only a teacher, but a friend; and yet at the same time the world feels itself richer, not only for the work, but for the example of this Christian gentleman and philosopher.

THE STAR DEPTHS.

A LECTURE

By RICHARD A. PROCTOR, Esq.,

B.A. (CAMBRIDGE),

Hon. Sec. of the Royal Astronomical Society.

Delivered in the Hulme Town Hall, Manchester, December 11th, 1872.

It would be easy for me to occupy far more than the hour or so allotted to this lecture, with the mere account of those facts concerning the stars which are to be found recorded in treatises on astronomy. But there would be a double objection to such a course. In the first place, I should be relating matters which could not but be known to many of my audience; and, in the second place, those among you who heard those matters for the first time could obtain such information far more conveniently and satisfactorily from the pages of one or other of our astronomical text-books. The course that I propose to adopt, then, in order to employ the short time at my disposal in the manner most likely, as I think, to interest you, is—not indeed to take it for granted that all those matters are known to you—but to speak of them without discussing at any length either their history or explanation, and so to make time to introduce to your consideration those parts of my subject which are more likely to be new to you, and to which my own researches relate.

If we contemplate the heavens on a calm, clear night, when all the stars shine, and

> The immeasurable heavens
> Break open to their highest,

the mind is impressed with the thought that perfect peace prevails amid those solemn depths. Thus the poet has, in all ages, found in the star-depths the aptest emblem of repose and stillness. Not

has astronomy, so far as it relates to the aspect of the heavens, taught any other lesson. It is true, indeed, that the astronomer recognises movement where to ordinary survey there is rest. He sees the star-sphere carried round day by day from east to west; he knows that as the year progresses the same motion can be perceived; while he recognises yet a third motion of the heavens, a motion carrying the whole celestial sphere gyratingly round an axis through the constellation of the Dragon in a period of nearly 26,000 years. But it is this rotating, revolving, and gyrating sphere which the astronomer has called the sphere of the fixed stars. For all these motions are but apparent. It is our earth, not the star sphere, which rotates once in each day; our earth which revolves once on its orbit in a year; our earth which gyrates like a gigantic top in the long period of 26,000 years, just mentioned. Her movements within the star sphere cause that sphere to change in aspect as though it moved around us, while in reality it is at rest.

To the general survey, then, of the astronomer, as well as to the contemplation of the poet, the star-depths present a scene of stillness and repose.

But so soon as we pass from these first considerations to study the teachings of modern astronomy more closely, we find that the heaven of the stars is in reality instinct with a vitality and energy, compared with which all the forms of life and motion known to us on earth sink into utter insignificance. The least of the stars visible to the unaided eye—some star so faint that on the darkest and clearest night it shows itself only by momentary twinklings—is in reality an orb full of life and energy; an orb giving forth during each moment of its existence greater supplies of force than all that are at work upon our earth during decades of years; an orb, in fine, resembling our sun in all those attributes which render him fit to be the beneficent ruler of a scheme of circling worlds. The least discernible change of brilliancy in any one of these orbs means an access or a loss of energy far exceeding the whole supply of light and heat which we receive from our sun in many thousands of years. And there is one circumstance we are apt to overlook, the consideration of which suggests strange conceptions respecting those seemingly silent depths amid which the stars are set. Our sun appears to supply us *silently* with light and heat, and with the other forms of force necessary to our well-being; but in reality the great central engine of our system works out its purpose amid noise, compared with which the loudest sounds known to us are as silence. The roar of the

hurricane, the crash of the thunderbolt, the bellowing of the volcano, and the hideous groaning which forms so terrible a feature of the earth-throe, all are transcended a millionfold by the uproar which must accompany the processes at work on every square mile of the solar surface. Now picture to yourselves that the tumult and uproar amid which our own sun beats out life to the worlds around him are repeated in every one of the thousands of suns we call the stars, in the millions on millions of stars revealed by the telescope, and in a million times as many suns which no telescope yet made by man can render visible. This is no idle dream or imagination, but has become, by the labours of our astronomers and physicists, a scientific certainty. It is the very charter of a star's existence that it shall be as a central heart, pulsating light, and life, and heat, and energy to the worlds over which it bears sway; that it shall be as a central engine whose roaring fires maintain the whole machinery of its system until the fuel which feeds them shall be exhausted.

At the beginning of the long series of researches by which these results have been ascertained lies the determination of the distances of the stars. So soon as the Copernican theory had been established, and it was known that year by year the earth circles around the sun on an orbit many millions of miles in extent, astronomers began to hope that the distances of the stars might be determined.

Before passing to the consideration of the conclusions which are to be drawn from the vastness of the stellar distances, I propose to indicate to you in a picture the basis on which astronomers have founded the determination of the sun's distance—on which these other determinations depend. Here, as in all cases where the distance of an inaccessible object is to be determined, change of direction, when the same object is viewed from different stations, is the point on which the determination rests. The stations in this case are on opposite sides of our earth, some 8,000 miles in diameter. There will now be shown on the screen a picture of our little earth, as supposed to be seen from the sun on the occasion of the transit of Venus, in December, 1874. You see Venus as a white disc in the centre of a triple circle, and below you see our earth at different stations, as if moving descendingly and towards the right past Venus, which planet, for convenience, is supposed to be at rest. It is from places at opposite sides of that tiny rotatory orb, which lies farther away than Venus (for, in fact, the two globes are of about the same size, whereas, as you see, Venus appears the larger), that astronomers

are to observe Venus as she crosses the sun's face. And now a picture will show you how much (or rather how little) Venus will be displaced. You have now on the screen a picture of the sun, and the path of Venus's centre across it (as supposed to be seen from the earth's centre) is shown by the central line of a set of lines crossing the sun's disc from left to right and ascendingly. The heavy shaded lines on either side of the central line indicate the zone along which Venus would seem to travel, if she could be viewed from the centre of the earth; the uppermost line shows the extreme limit of the zone she would traverse if she could be viewed throughout from the most southerly place on the earth; while the lowest line shows the extreme limit of the zone she would traverse if she were viewed throughout from the most northerly place on the earth. You perceive that the displacement is by no means great: nevertheless it has been measured on former occasions of the kind, and we know certainly from these and other observations that the sun's distance is about 91½ millions of miles, and the span of the earth's orbit twice as great.

It seemed certain, then, that the nearest among the stars would be seen in a different direction when the earth was at any given point of her orbit than when she was at the opposite point, 183 millions of miles away. Nevertheless, even this enormous base line has proved barely sufficient, in conjunction with the use of the most delicate and powerful astronomical instruments, to exhibit the minutest measurable displacement of two or three of the nearest stars. To show how extremely delicate is the problem thus attacked by astronomers, I will indicate the actual displacement of direction of the nearest of all the stars (so far as known), the star Alpha, of the constellation of the Centaur. You all know how slightly the minute-hand of a clock or watch moves in a single second of time—it is barely possible to recognise its change of direction. Now, in a second the minute-hand of a clock or watch changes in direction four hundred times as much as a line pointed to the star Alpha Centauri changes in direction while the earth circles on its orbit, though that orbit has a span of 180 millions of miles. If you conceive that star as the centre of a gigantic clock-face, and a line from it to the sun as the minute-hand of a mighty clock, then the length of that hand would be so enormous that its end would move over 180 millions of miles, not in an hour, nor in a minute, nor in a second, but in the four-hundredth part of a second. Two stars are known (in one case the knowledge has come quite recently) to lie at about twice the distance of Alpha Centauri. But it affords at once an indication of the tremendous

difficulty of the problem, and of the uncertainty which even now surrounds its solution, that one of these stars (a celebrated, though small star, known as 61 of the Swan) was set until lately at about three times the distance of Alpha Centauri, instead of rather less than twice that distance—a trifling error truly when the actual mistake in the estimated displacement of the star is concerned, but nevertheless an error in distance amounting to some two hundred thousand times the distance of our sun. It may be safely said that Alpha Centauri is the only star whose distance can be regarded as even approximately determined; and even in the case of that star the probable error amounts, not to a few millions of miles, but to some thousands or tens of thousands of millions. But it must be remembered that this circumstance introduces no doubt whatever as to the fact that *all* the stars lie beyond distances such as we have been considering. It may be regarded as practically certain that there is not a single star in the heavens lying at a distance less than two hundred thousand times that of the sun, while the great majority, all in fact save perhaps some ten or twelve, lie at distances enormously greater.

Now, since the nearest of the fixed stars is more than 200,000 times as far away as the sun, it follows that if the sun were removed to the place occupied by such a star, his light would be reduced, not 200,000 times, but 200,000 times 200,000 times, or 40,000 million times. But the light of Alpha Centauri has been measured, and has been found to be equivalent to about 17,000 millionth part of the sun's! So that even if we set this star at only 200,000 times the sun's distance, its light exceeds the sun's more than twofold. But, according to the best estimates of the star's distance, it must emit about three times as much light as the sun. Accordingly, if it is a globe, whose surface gives out as much light, mile for mile, as the sun's, then its surface must be three times as great as the sun's: whence it is easily shown that its diameter must be more than half as large again as his, and its volume about five times his. As a matter of fact, the star is double, and the companion gives out one-seventh, or thereabouts, of the total light supplied by the pair. But this leaves the larger star still far greater than our sun in bulk—certainly fully four times as great, on the supposition I have made as to the star's light.

Setting all assumptions of this kind on one side, it is certain that our sun, placed where Alpha Centauri is, would shine only as a leading second-magnitude star; placed twice as far away, or where 61 of the Swan lies, our sun would be reduced to the

brightness of a medium third magnitude star. Now these, be it remembered, are the distances of the very nearest of the stars. We have only to set our sun at the greater distance at which astronomers set Sirius, the brightest star of all in the heavens, to find him reduced to a low fourth-magnitude star; while, at the distance where lie, most probably, the greater number of the leading orbs—Vega and Arcturus, Altair and Betelgeux, Rigel, Aldebaran, and Antares—our sun would resemble those faint stars which can only be seen on the clearest and darkest nights.

But it may be urged that, after all, though the stars are thus seen to be lights as brilliant as our sun, and many of them far more brilliant, doubt may yet remain whether they are in other respects like him. The idea has indeed been entertained by eminent students of science that the stars may be *mere* lights, not vast and massive orbs like our sun, capable of swaying the motions of schemes of dependent worlds. And a quarter of a century ago it seemed impossible to show that this theory, strange though it might appear, was unsound. But that wonderful method of research—spectroscopic analysis—has definitely removed these doubts, proving beyond all possibility of question that the stars are suns; not, indeed, constituted in all respects like our sun, but resembling him in all essential characteristics.

It is fortunately (considering the progress of time) unnecessary for me to enter here into a description of the methods and results of spectroscopic analysis, for you have heard those methods and results explained in this room by the very ablest expositors of the subject,—Professor Roscoe and Dr. Huggins. But I must touch, in passing, on those points which relate specially to the star-depths, since otherwise the evidence I am adducing would want an important link.

You know that prismatic analysis resolves the light of our sun into a rainbow-tinted streak, crossed by multitudinous dark lines; and that the lesson taught us by this fact is that the sun is a mass of glowing solid, liquid, or very densely compressed vaporous matter, shining through vapours less intensely heated, though still very hot. It is these vapours which produce the dark lines. Hence, if it be shown that the spectrum of a star resembles the sun's in these general features, we learn that the star is constituted in like manner.

Now there will be thrown upon the screen the various forms of spectra which Father Secchi, the Italian astronomer, has recognised among six hundred stars which he has examined. I do not say that you are to attach any great weight to this classification;

but it serves well enough to indicate the general nature and varieties of the stellar spectra. You see, in the first place, that *all* the spectra afford the kind of evidence I just now mentioned – all these spectra are crossed by dark lines; and therefore all the stars examined by Secchi are masses whose light shines through extensive vaporous envelopes. All, therefore, are *suns*, not mere lights.

Proceeding to details, you see that the uppermost spectrum is the largest, and it should be the brightest, but is not so shown in the picture. It is the spectrum given by Sirius, Altair, Rigel, Vega, and other stars, mostly distinguished by their great brightness, and all characterised by a somewhat bluish tint. The spectrum is crossed by four very strongly-marked dark lines, which are those corresponding to the gas hydrogen. Of the six hundred stars examined by Secchi, about three hundred gave a spectrum of this order. There are reasons for believing that the stars giving this spectrum are much larger than our sun, and have vaporous envelopes much deeper than his. Sirius, certainly, the only star of the kind whose distance has been even roughly determined, is from 1,000 to 8,000 times as large as the sun—supposing his size is to be estimated by the quantity of light which he emits. We may infer, as at least highly probable, that these stars are not simply larger than our sun, but belong to a higher order of suns altogether.

The second spectrum is that of a star resembling our sun in constitution. It may be regarded, in fact, as the spectrum of our sun in miniature. Secchi found that about 150 of the 600 stars he examined gave a spectrum of this order. Among the stars belonging to this class are Capella, Pollux, Alpha in the Great Bear, Procyon, &c.

The third and fourth spectra indicate the extreme range of the various forms under which star spectra of the third order are seen. Those spectra of this class which differ least from the second spectrum correspond very closely (according to Secchi) to the spectrum of a sun-spot; and he therefore infers that the stars of this order are suns covered with many spots. Amongst these stars Secchi includes Antares (the Scorpion's Heart), Betelgeux, the famous irregular variable on the shoulder of the Giant Orion; the star Mira, or Wonderful (the even more celebrated variable in the Whale); and other stars.

The fourth spectrum is that presented by certain red stars about thirty in number), chiefly inconspicuous.

In passing to the work of Dr. Huggins and the late Professor

Miller, I must not omit to notice that although Secchi examined a greater number of stars, the work of our two English physicists is of far greater real weight and importance. They were not content with the mere general survey of star-spectra, but instituted a careful analysis of the individual lines of different spectra, comparing the places of these lines with those of the bright lines obtained from various elements. Thus they were able to announce with confidence that certain elements, familiar to ourselves, exist in the vaporous envelopes surrounding stars which they examined.

The next picture brought upon the screen illustrates the work of these eminent men.

You see on the screen three coloured spectra. The upper most shows the solar spectrum, with its principal lines. The next is the spectrum of the star Betelgeux, the bright star on Orion's shoulder. The lowest is the spectrum of the ruddy Aldebaran. To begin with this star: you perceive the great number of dark lines shown on this spectrum; these, however, are only the lines whose position the observer actually measured; many more were seen. Now below the spectrum are many bright lines. These are the lines obtained from different elements, whose spectra the observers compared with that of the star. Whenever a set of these bright lines agreed in position exactly with a set of dark lines in the spectrum of the star, the observers knew that the corresponding element exists in the strata of absorbing vapour in the atmosphere of the star. They tried in this way sixteen elements, and satisfied themselves that nine of these exist in Aldebaran's atmosphere. These elements are — the metals iron, bismuth, antimony, mercury, sodium, magnesium, calcium, and telluriun, and the gas hydrogen. In the case of the star Betelgeux, they determined the presence of five elements, viz., sodium, magnesium, bismuth, calcium, and iron. The hydrogen lines could not be recognised in the spectrum of this star. You will understand that the elements here mentioned are those which may be regarded as certainly present in these stars; but that those elements whose lines have not been recognised are not therefore to be regarded as necessarily absent. Especially it is to be noted that hydrogen is not probably wanting in the star Betelgeux, though its characteristic lines are not seen. It is probable that either the hydrogen envelope is simply too shallow to produce atmospheric lines which the spectroscopist can recognise, or else the hydrogen in Betelgeux exists at so high a temperature that its light is as effective as that of the glowing matter beneath, so that

it fills up the space where its dark lines would otherwise show with light of the same brightness as the rest of the spectrum, and therefore shows no lines. It is indeed noteworthy that Betelgeux is a variable star, and that possibly the spectrum may be found to vary as respects the brightness or darkness of the hydrogen lines. In fact, spectroscopic analysis, which tells us what elements exist in the stars, is capable also of supplying information as to the condition in which those elements exist.

In passing, I may remark that Dr. Huggins has found that some stars give a spectrum in which the lines of hydrogen are not dark as in the spectrum of Aldebaran, or wanting as in that of Betelgeux, but are bright. Notably was this so, as you will probably remember, in the case of the spectrum of that wonderful star T of the Northern Crown (of which Mr. Baxendell was one of the discoverers), which suddenly blazed out in May, 1866.

As another illustration of the kind of information which spectroscopic analysis can give respecting the condition of stars, I will cite the case of those beautiful objects, the coloured double stars. A picture will be brought on the screen showing one of these objects—the star Albireo in the Swan. You perceive that the brighter star is of a strong orange colour, while the smaller is beautifully blue. Now we might be in doubt whether these stars shone with inherent orange or blue light, or whether their light appears orange and blue on account of the nature of vapours through which it shines. Spectroscopic analysis at once answers this question. You see now on the screen the spectra of these two stars. Both spectra show the complete range of the spectral colours, and we hence learn that the inherent light of both stars is white. But the spectrum of the orange star—the lowest in the picture—shows several strong dark lines in the blue. Hence a considerable proportion of the blue light of the star is cut off, and an excess of light from the red, orange, and yellow parts of the spectrum remains, so that the star appears orange. The spectrum of the blue star, on the contrary, shows a number of dark lines in the orange, so that there is an excess of light from the blue end of the spectrum, and the star appears blue.

Yet another illustration of the teachings of spectroscopic analysis respecting the star-depths :—Besides the stars, there are in the heavens many cloud-like objects, some of which have been found to be composed of multitudes of stars, while others had remained of a doubtful nature. Now, the spectroscope tells us that many of these cloudlets, or nebulæ as they are called, really consist of suns like our own, since they give a spectrum resembling

the star spectra you have seen. But others are of a totally different nature. One of these is now shown upon the screen. It is the famous ring-nebula in Lyra. The spectrum of this object will now be shown. You perceive that it is not a rainbow-tinted streak like those which were before shown, but consists of three bright lines. Dr. Huggins has already told you what he has ascertained about these lines. My subject only requires me to mention that the spectrum shows this object to be formed of glowing gas. You will presently see that this discovery has an important bearing on the ideas we are to form respecting the constitution of the stellar system.

I will now pass on to discuss another circumstance in the condition of individual stars. We have seen that the stars are suns like our own in all essential matters. And thus we can apply to them what we have learned respecting our sun, and infer that processes are at work in the stars resembling those wonderful processes which we know to be at work in our own sun. But, as we are thus enabled to apply our knowledge respecting the sun in order to make inferences respecting the condition of the stars, so from our study of the stars we can form inferences respecting one important circumstance, at least, in the constitution of our sun. It is a matter of great interest to us to determine whether our sun has, in former ages, had the same brilliancy as at present, and whether he is likely, during the coming centuries, to emit, without diminution or increase, the light and heat necessary for the wellbeing of the worlds which circle around him, Now it is certain that many stars vary in lustre either periodically, or, so far as is known, irregularly. If it should appear that most of the stars are subject to irregular variations of brightness, the inference would certainly be that our sun is likely to change likewise, for he is one among the stars. Precisely as we should feel anxious in travelling on a railway where accidents were common, so the astronomer might feel doubtful about the sun's steadfastness, as a source of light and heat, if change were common among the suns. Now, in order to answer the question thus suggested, a very careful comparison of the brightness of the stars has to be undertaken. But, even when this has been done, we must not expect that in the course of a few years, or even of a few centuries, we shall be able to form an opinion. Several good observers in our own day have made systematic observations of the stars, and have, in fact, discovered many variable stars, though few compared with the total number of stars. Amongst these observers not one has achieved greater success than your distinguished townsman, Mr.

Baxendell. Now these observations will enable the astronomers of a future epoch to determine whether variation is the common fate of stars, or whether constancy is the more general law. But before this can be done many years must elapse—probably many generations of astronomers must pass away. We are led, then, to inquire whether any method exists for obtaining a readier, if a somewhat less satisfactory, solution of the problem. I think that such a method exists in the study of those seemingly absurd figures, the constellations. If it be admitted that the first observers of the heavens really recognised the figures of men and animals in those star-groups to which they gave corresponding names, it would be interesting to inquire whether such star-groups retain a sufficient resemblance to those figures to account for the names they bear. Now there can be little question, I think, that the ancients did recognise certain resemblances, for some constellation names are common to many different nations and races. Amongst such constellations may be mentioned the Bear, the Lion, and the Ship. Now, there will be thrown on the screen a map of the northern constellation-figures. Probably it is on too small a scale to be seen from many parts of this hall. But those who cannot perceive the figure of the Bear, know, nevertheless, that in our modern maps the Bear appears as a long-tailed animal, whereas, as every one knows, the Bear is short-tailed, one may almost say tailless. Now those who first called this star-group the Great Bear, must have known perfectly well all the chief peculiarities of the bear's shape. It is indeed a rather singular circumstance that we find among the Egyptians (who would not be familiar with the bear) that the same star-group was named after the hippopotamus, an animal resembling the bear in being short-tailed, cumbrous, small-headed, heavy-muzzled, and short-eared. None of these characteristics can be recognised in the modern constellation of the Great Bear.

Again as to the Lion. A map will now be shown in which you will see the modern division of the stars including the constellation of the Lion. You perceive a sickle-shaped group in the middle of the mass. Well, the Lion's head in our modern maps corresponds to the stars forming the point of the sickle, a group utterly unlike a lion's head. The distinguishing characteristics of the lion are his fine head and mane, his mighty limbs, and his long tufted tail. None of these features can be recognised in the modern constellation of the Lion. Now a map will be thrown on the screen showing the actual star-grouping where the ancients recognised the Lion and

the Bear. Near the top you see the well-known seven stars of the Bear. Now I think that if instead of regarding those three stars which form the Bear as forming part of the outline of the animal's hind-quarters, you may begin to recognise the configuration of an animal larger than the modern constellation figure of the bear, and not unlike a bear. The claws are well indicated by three bright triangular sets of stars. The small short-eared head would occupy the upper left hand part of the map, and though the star-group here (being formed of small stars) does not show well in a star map, it does in reality somewhat strikingly resemble the peculiar shape of a bear's head. Below is the Lion; and I think that, by the exercise of a little imagination, you can recognise the shape of a lion's head on the right, the outline of the mane (formed by stars corresponding to the Bear's claws), the figure of the body, and here, in this group of stars which modern astronomers call the Hair of Berenice, you have the tuft at the end of the lion's tail. It agrees with this view of the matter that the Arabians did actually call Coma Berenices the Lion's tuft. Now, in the next picture you see the figures of the Bear and the Lion as I conceive that the ancients recognised those figures on the heavens: I think you can see that all the more remarkable parts of the star-grouping are accounted for in the two figures.

The inference from this would be that at least the greater number of the stars in this part of the heavens have remained with little change of brightness; and that the real change has been in our method of figuring the constellation, not in the stars. However, in the very region of the heavens which has led us to this encouraging result, there is a star which has changed so remarkably as to show that we cannot certainly trust in the continuance of the sun's light and heat. It is the star, Delta of the Great Bear, the middle star of the well-known seven. This star was as bright as the others two centuries ago, and has now sunk to the fourth magnitude. There can be no doubt that if a corresponding change took place in the brightness of the sun, nearly all the creatures living on our earth would perish.

I pass on to another illustration of this method. In the map next shown you see the figure of the Ship Argo on the right, and close by is the figure of the Greater Dog. Next a map is shown in which you see how the stars are at present divided between Argo and the Dog. But I think you can perceive that the stars in this map form a figure resembling the stern of an ancient ship, if only the constellation is not limited by the modern boundaries. This is better shown in the next map, where the boundaries are

removed; and in yet another map you see the figure of such a ship as it was undoubtedly recognised by the ancients—the stern-half, namely, of a large ship, with the stern towards the west. Here, then, again, I think that we have reason to believe that at least the greater number of the stars in question have changed little in brightness; and that it is because our method of dividing the stars into constellations, differs from the ancient method, that there is often a want of resemblance between the constellations and the figures with which they are associated. But in this instance again, as in the former, there is a star whose history should teach us not to be absolutely certain that our sun's lustre and heat will remain without change. I refer to the celebrated star, Eta in the Ship, which only a quarter of a century ago was shining more brightly than any star in the heavens, except Sirius alone, while it is now so faint as to be barely discernible on the darkest and clearest night. There can be no doubt that if our sun thus changed in brightness, until he shone but with one-hundredth part of his present lustre, every form of life would perish from off the face of our earth.

Let us now turn to the consideration of the various theories which have been formed respecting the constitution of the stellar system.

First there will be shown on the screen the star sphere as it was conceived by Kepler. You see in the middle the solar system; then around that there is a ring, which represents a section of the sphere of the fixed stars. By ingenious, but quite untenable reasoning, into the nature of which I need not here enter, Kepler was led to the conclusion that the star-sphere is about seventy miles in thickness.

The next picture presents the theory of Wright, of Durham, commonly ascribed to Sir W. Herschel. You see in the middle his conception of our star system (shown only in section)—a disc of stars somewhat uniformly scattered. Wright explained the Milky Way as the region towards which this disc of stars has the greatest expansion, so that when we look in its direction the line of sight passes through a long array of stars, producing a cloudy light by their united lustre. The disc is shown cloven, to correspond to the fact that the Milky Way is divided along one part of its length into two streams. Around it are other systems of stars differently shaped—some disc-like, some ring-shaped, some spiral, and so on.

Next a picture is shown of the stellar system according to the ideas of Lambert. Again we find the figure (in section) of a cloven disc, but instead of the stars being uniformly scattered

throughout this disc, they are arranged, according to the theory of Lambert, into separate clusters, generally globular in shape.

I pass over the labours of the Herschels, as forming a subject altogether too wide to be discussed in the brief time now at my disposal. I note only that according to the theory ascribed to the elder Herschel by our books of astronomy, the stars are arranged as in the theory of Wright, that is, somewhat uniformly within a space shaped like a cloven disc.

William Struve, the eminent German astronomer, recognised the fact that if this theory be true the stars of the leading orders of magnitude ought to show no tendency to aggregation along the zone of the Milky Way. Taking the catalogue of Weiss, containing about 32,000 stars of the first seven orders, he found that they show a marked tendency of the kind. And he announced accordingly his belief that the stars are not spread uniformly throughout the galactic disc, but are gathered towards the central region or mean plane of that disc. His theory is illustrated in the picture next thrown on the screen, where, as you will perceive, there is no longer a uniform scattering of the stars, but a gathering towards the central line of the galactic cloven disc (again shown in section).

I now pass to the results to which I have been led by my own researches. The purposes I have had in view during my inquiries have been mainly these: First, to proceed, in perfect independence of all preconceived theories, to inquire how stars and star cloudlets are spread throughout space, how they differ in magnitude, motion, constitution, &c., and what laws govern their changes and movements, instead of adopting assumptions on these points as bases for reasoning; and, secondly, to endeavour in every case to render clear to the eye those relations which hitherto (where they have been discovered at all) have been presented only in catalogues or tables of stellar statistics.

First, I have applied to stars of different orders of brightness processes of equal-surface charting, which serve to show how the stars are distributed. In the map now shown are all the stars of the northern hemisphere to those of the fourth magnitude. You perceive that even among the stars of these, the brighter orders, there is no uniformity of distribution, but that they are richly spread in parts, while elsewhere there are regions where they seem wholly wanting. The next picture shows four parts of the heavens, equal to each other in extent; and the stars down to the fifth magnitude only are shown. You perceive that there is a great difference in the number of stars presented in these four equal

spaces.* You can also notice in all four maps that there are well marked streams and aggregations of stars. The fourth of the charts includes the constellation of the River, which (as its name implies) includes a stream of stars recognised by the ancients. Next another map, showing the self-same region, but including stars down to the sixth magnitude, will be brought on the screen. Now there you can perceive that there is a gathering of the stars towards the lower part of the map. It would be interesting then to see what would be the effect of showing in a single map—or, perhaps, first in two maps, one northern one southern—all the stars down to the sixth magnitude. Now the next two maps have been constructed for this purpose. In the first we have all the northern stars visible to the unaided eye on a dark, clear night. You can see at once that there is not the general uniformity of scattering commonly believed in, but that there is a certain large region where the stars are aggregated with much greater richness than elsewhere. In the next, or southern map, you have a similar phenomenon, but much more markedly shown. The rich region here is a part of the heavens where stars are spread so densely that, according to the account of Captain Jacob (a late well-known astronomer), the sky is filled with light as from a young moon when this part of the heavens is above the horizon. I think it will be admitted that the density of aggregation here is not the effect of mere chance distribution, but indicates a real aggregation of the stars into certain portions of space, and their segregation or withdrawal from others. This is perhaps even more clearly shown in the next picture, in which are included all the visible stars in the heavens—that is, all which were shown in both the last maps. You can there see the rich northern region as well as the rich southern region, while between them lies a well-marked zone of darkness, where scarcely any stars can be seen.

It is worthy of notice that the richness of star-gathering is not only shown in those two regions, but even more markedly in the Milky Way, even though the Milky Way crosses the zone which is so poor in stars. I have found, indeed, that the stars in the Milky Way are so richly spread, that, if the whole heavens were covered with equal richness, there would be about nine thousand more stars visible to the eye than can actually be seen. On the other hand, to show that it is the galaxy itself, and not the galactic region, which is thus rich in stars, the dark gaps and spaces within

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* A great number of charts were shown on the screen, the teachings of which cannot, of course, be satisfactorily indicated by mere verbal description.

the Milky Way are actually so poorly strewn with stars, that, if the whole heavens were similarly bestrewn, there would be four thousand fewer stars than are now visible.

It seemed to me that it would be desirable to extend this process of star-guaging farther. Having Argelander's splendid series of forty large charts of the northern heavens, containing in all 324,198 stars, I determined to map these in a single equal surface chart. These maps include all the stars down to the tenth magnitude inclusive, and, according to the theory of the elder Herschel, no signs of a special gathering of these stars on the galaxy ought to be recognised. I shall leave you to judge whether this is the case or not. We will first of all have the map brought on the screen in seven successive parts, so that we may have a large scale. [This was done, and the maps were described as they were placed in succession on the screen. The point chiefly dwelt upon was the gathering of the stars towards the Milky way.] You have now seen that wherever the Milky Way crosses any part of one of these maps, the stars are strewn with greatly increased richness. We will, now, however, have the whole map on the screen at once. It is now before you, and you can see that the place of the Milky Way is shown with perfect distinctness, merely through the exceptional richness of stellar aggregation along that zone.

I will now pass from the stars to the star cloudlets, and show the evidence I have obtained as to the distribution of these objects. I have applied to them the same process of equal-surface charting which I applied to the stars. There will now be placed upon the screen a map showing the distribution of all the star cloudlets known to astronomers. You can see that the arrangement is somewhat remarkable. Over the Milky Way, where, as we have seen, stars are more richly spread than elsewhere, nebulæ are almost wholly wanting, while they are clustered elsewhere. It seems as though along the galaxy all the star material had been, as it were, used up in making stars; while elsewhere material had been left whence star-clusters could be formed. You may wish to see these great clustering aggregations separately. I therefore will now have two other maps brought upon the screen showing them. You will observe that from the great central mass of aggregating nebulæ streams of nebulæ extend, both in the northern and southern hemispheres, but more markedly in the southern. It is very noteworthy that where those two more remarkable southern nebular-streams appear, there are two of the most remarkable star-streams, viz., that seen in the constellation of the River before mentioned, and the stars which form the streams

from the water urn of Aquarius. Yet more remarkable is the fact that each of these intermixed streams of stars and of nebulæ passes on until it loses itself in a great cluster of intermixed nebulæ and stars—the Greater and Lesser Magellanic Clouds.

It seems to me that all this evidence proves, beyond all reasonable question, that the nebulæ are associated with the stellar system, though what may be the exact nature of the association is not as yet clear to us. It seems to me that this is further indicated by the way in which many of the nebulæ seem to cling around groups of stars. Several illustrations of this peculiarity will now be brought upon the screen. You will see that in many instances the association is too remarkable to be attributed to accident. This is especially the case in the nebulæ around the stars c_1 and c_2 of Orion (shown on the screen); for here we have two double stars, each occupying the exact centre of two well-marked nodules of the nebulæ.

Further to test this matter, I determined to map on a large scale all the nebulæ in two regions of the heavens, where these objects are spread with exceptional richness, and to include in the same map all the stars down to the tenth magnitude. One is the great nebular region in the constellation Virgo. It is now shown on the screen, and those among you who are nearest can recognise a peculiar law of association between the nebulæ and the stars; for the stars are gathered into streams, and very few are seen in certain parts of the map. Now, it is not where few stars are seen that many nebulæ are seen; but, on the borders of the star-groupings, and in the places where the star-groups seem incomplete for want of a star or two, nebulæ are found so placed as to suggest the idea that they represent the missing stars. Another map showing the rich nebular region in the constellation Coma Berenices, is now before you. It teaches precisely the same lesson as the former.

Before passing from the subject of the nebulæ, I may mention that, as there are variable stars, so also there are variable nebulæ. In the picture next brought on the screen you see two views of the variable nebula surrounding the remarkable variable star Eta Argûs; and, as you will see, the two pictures appear to indicate a somewhat considerable variation.

I pass now to the motions of the stars. Astronomers, carefully comparing the present positions of the stars with their positions as observed by the astronomer Bradley in the last century, have recognised slight changes of place. These slight changes indicate real motions of enormous velocity. It appeared to me that it

would be well to show in some graphic way how the stars were moving. I have accordingly drawn charts of all the stars which have been compared in the way I have mentioned, and I drew a small arrow from each star to show the direction in which the star is moving. The two charts now shown indicate the general result of this process. They include about 1,500 stars. You perceive that the motion-arrows of many of the stars are very large, while other stars are moving at a comparatively slow rate. I may mention that these arrows show the amount of motion of the stars in 36,000 years: that is, each star at the end of about that time will be in the place occupied by its arrow's point instead of its present position. This will give you an idea of the extreme slowness of the apparent motion: nevertheless, the real motion is in many cases known to be very rapid—several miles per second, for instance.

One of the objects which I had in view in applying this process of charting was to determine whether the stars which seemed from my other maps to show a tendency to grouping together, might not show also a tendency to drift together, as groups of stars might be expected to do. I recognised several instances where, as it seemed to me, this tendency to star-drift is very strongly shown. I select two cases of different kinds. The first case, illustrated by the map now before you, relates to the motions of the stars in Gemini and Cancer. You can see that nearly all the stars included in the map are travelling in the same general direction. In the next map you have the proper motions of the stars in the Great Bear. You will notice that of the seven principal stars (the familiar stars of the Plough), five are travelling in the same direction and at the same rate. It is a noteworthy circumstance that the direction of their drift is exactly opposite that which would be due to the sun's motion in space. It seemed to me so clear that we have here an instance of true star-drift (and if we can accept this case as proved we shall be ready to admit the other cases where the evidence is less perfect), that I ventured on a prediction respecting these stars. I knew that my eminent friend, Dr. Huggins, was about to apply to these, among others of the brighter stars, a method of spectroscopic research, by which he is enabled to determine at what rate a star is receding from or approaching the earth; and I announced, three years ago, my belief that whenever he applied this method to the five bright stars in the Great Bear, which are here shown as drifting together, he would find that they were either all approaching or all receding, and at a common rate. This prediction has been exactly

confirmed by the result. Of the other two stars of the seven, one is approaching, the other is receding at a moderate rate. *All the five bright stars enclosed within that curve on the map** are receding at the common and enormous rate of seventeen miles per second. They also all show the same spectrum, indicating that they belong to the same order—the order, namely, to which Sirius, Rigel, Vega, and Altair belong.

The general conclusions to which I have been led by the methods of research which I have described are briefly these: First, the sidereal system is altogether more complicated and more varied in structure than has hitherto been supposed. The picture now on the screen is intended to indicate (necessarily in a very rough and coarse manner) my ideas as to the constitution of the heavens. It seems to me that within certain regions of space stars of many orders of real magnitude are gathered together. All the star cloudlets hitherto discovered, gaseous or stellar, irregular, planetary, ring-formed, or elliptic, exist within the limits of the sidereal system. They all form part and parcel of that wonderful system whose nearer and brighter parts constitute the splendour of our nocturnal heavens.

It has been supposed that my views tend to reduce our ideas as to the scale on which the universe is constructed. The exact reverse is, however, the case. It is true that I cannot recognise external galaxies in the star cloudlets—that I recognise parts of our system where it has hitherto been believed that outlying universes are in question. But I reason thus because I have been led to the conclusion that our sidereal system is much more extensive than has been hitherto supposed. It is not that I draw the nebulæ inwards to the star depths, but that I extend the star depths outwards, so as to include the nebulæ.

In concluding, I would address to you a few words on the wonderful scene presented by the star depths. Let our thoughts pass from our earth, which seems so magnificent, to the giant orbs of Saturn and Jupiter, which dwarf her dimensions to insignificance; thence to the sun, compared with which the largest of the planets seem so small. Then let us consider the dimensions of the solar system, compared with which even the dimensions of the sun are as nothing. Next let us pass on in thought to the vast region of space within which our whole solar family is travelling.

* It should be mentioned that the map to which these remarks are applied was not drawn after the prediction had been fulfilled, but had appeared in the lecturer's treatise, "Other Worlds than Ours," published two years earlier.

Then let us picture the scheme of suns of which our sun is a member;—not the sidereal system, scarcely even an appreciable fraction of that system, but the particular family of suns to which the sun belongs—and let us consider how the domain of the sun, the region of space over which he bears sway, is in its turn reduced to mere nothingness by comparison with the scheme of suns of which our sun is a member. Then, lastly, let us picture to ourselves that the scheme of stars to which our sun belongs is but one of the atoms of which the frame of the sidereal systems is built.

We can speak of these things, but we cannot conceive them. The astronomer can spread out the figures which represent these wonders, but he can neither enable others to conceive them, nor can he conceive them himself. I know not then how I can more fitly draw my subject to a conclusion than by quoting the wonderful dream in which the German poet, Jean Paul (nobly translated by our own prose poet, De Quincey), pictures the feebleness of human conceptions in the presence of the infinite wonders of the universe:—

"God called up from dreams," &c.

KENT'S CAVERN.

A LECTURE

By WILLIAM PENGELLY, Esq., F.R.S.

Delivered in the Hulme Town Hall, Manchester, December 18th, 1872.

I NEED not inform you, ladies and gentlemen, that there is in South Devon an inlet of the sea known as Torbay, and that on the northern side of it there stand the town and harbour of Torquay. About a mile eastward from that harbour there is a beautiful valley—that I need not have said; all valleys in Devonshire are beautiful—running north and south, and terminating southwards in Torbay. About half a mile from the bay there enters that valley a transverse valley on the western side; and at the junction of the two there is a low wooded limestone hill, isolated from the surrounding country, which surrounding country rises to a higher level, but is not limestone; and on the eastern side of that limestone hill there is a low vertical cliff about thirty feet in height, and two hundred feet above the sea, in the face of which there are two apertures, into either of which, if you choose to go, you will soon find yourself in a large, dark, dirty hole, known as Kent's Cavern. You have been so kind as to leave your pleasant firesides this evening in order to give me an opportunity of talking to you for about an hour on this same dirty hole.

It is one of my disadvantages to be almost a stranger to every one in this room. I say disadvantage, because I take it for granted that I am not known to you as a lecturer at all; and consequently I ought, as a matter of fairness to you and to myself, to state one or two of my defects as a lecturer: you will discover the others as you and I go along. First of all, I am in the habit

of talking very rapidly. I trust I may not offend in that direction to-night. Secondly, I have a foolish habit—I admit it is foolishness, but I am too old to get into a new groove now—I have a foolish habit of supposing that there is in some corner of the room a modest and retiring boy desirous of information; and I frequently suppose that boy to be my audience, and address myself to him, forgetting that all of you know all about the subject just as well as I do. If I should fall into that error, and explain anything to my friend in the corner, I hope you will pardon me.

Why this cavern is called "Kent's Cavern" I cannot tell, nor can anyone else. It seems to have been known from time immemorial: there is no tradition of its discovery: there never was a time when it was not known, as far as we can make out. The oldest mention I have seen of it occurs in a map still existing at Torquay, dated 1769, which is a map of the Torwood property, where the cavern is situated; and on the map there is one field or portion of ground entitled "Kent's Hole Field." It had given the name to that portion of the property; from which we may conclude that it was very well known prior to 1769. The earliest known description of it occurs in a MS. work, never yet published, by a Mr. Swete, a well-known Devonshire name, which work was written in 1792.

With reference to the name, the late Mr. Babbage told me that when a boy he was at Torquay a good deal, that he frequently visited the cavern, and that the guide informed him that the name originated in this way. I do not ask you to believe it; you can please yourselves about that. A dog was taken into the cavern and lost there; it was subsequently discovered in the county of Kent, from which it was concluded that there was a subterranean passage from Kent to the cavern; and hence the name. If that does not satisfy you, I dare say you can invent a reason for the name as good as that. I ought to tell you that the earliest account of this story makes it out that it was a hawk that was taken into the cavern. I mentioned this in London two or three weeks ago, and the reporter (who had the good sense—not that I mean this as a hint to reporters in general—to send me his report, lest he should have made any mistake), owing to my thick pronunciation, I presume, stated that it was a *horse*, not a *hawk*.

The cavern has been one of the "lions" of the district from at least 1792, downwards; but it was not till 1824 that anyone thought of seeking bones in it. Then a Mr. Northmore, a resident near Exeter, having the idea that every cavern was a temple of Mithras, came to Torquay, resolved to discover that Kent's Cavern

was such a temple. And it is a curious fact that when a man is resolved to discover a thing, he frequently succeeds; at any rate, he saw things which he could twist this way and that way, and make out it was such a temple. It happened that the day before he went into the cavern some one had put into his hands Buckland's description of the Kirkdale cavern, in Yorkshire—(another hint for the reporters: in London, when I mentioned this, the reporter put it down the "Crocodile Caverns.") And then it occurred to Mr. Northmore that he perhaps might find bones in Kent's Cavern. He dug, and succeeded in finding bones. That was in the summer of 1824. Sir Walter Trevelyan followed, and found some bones also in the same year. But in 1825, Mr. Northmore, on going there again, was joined by a gentleman residing at Torquay at that time, the Rev. John MacEnery. Now here again, ladies and gentlemen, I must stop to digress. When I pronounced that name in Brighton last August, the reporter, supposing me to be a cockney, and that I had dropped the letter *h*, corrected it, and put it Henry; nevertheless it is Enery. The Rev. J. MacEnery was a Roman Catholic clergyman, a very highly educated man, and he was induced to accompany Mr. Northmore to the cavern in 1825. He knew nothing about geology. I have sometimes thought that nothing could be more instructive than to get at a man's emotions and feelings, if you could, when a new thought first occurs to him, or a new discovery flashes across his mind. It is like raising a spirit which it is beyond your power to lay again. And MacEnery has told us somewhat of his feelings on this occasion. "The passage," he says, "being too narrow to admit more than one person at a time, * * the company entered in files, each having a light in one hand, and a pickaxe in the other, headed by a guide, carrying a lantern before the chief of the band. I made the last of the train, for I could not divest myself of certain undefinable sensations, it being my first visit to a scene of this nature."

He made good use of that first visit. He soon found that Mr. Northmore was not a man likely to take philosophical views of things. He, therefore, separated himself from the company, went into a little recess, and, working quietly by himself, had the good fortune to disinter fossils. I must trespass again on your patience whilst I read a passage from his work on that discovery: "They were the first fossil teeth I had ever seen, and as I laid my hand on them, relics of extinct races, and witnesses of an order of things which passed away with them, I shrank back

involuntarily. Though not insensible to the excitement attending new discoveries, I am not ashamed to own that in the presence of these remains I felt more of awe than of joy."

I think it is extremely interesting to see this virgin mind going into what was almost a virgin cavern—to see a man of high culture, with few theological prepossessions, and having the greatest possible love of truth—going into this cavern with the intention of doing his best to find what the cavern contained. He undertook after awhile to make a systematic exploration of certain portions of it, or what he considered to be a systematic exploration, but with which we should hardly be satisfied in the present day. He died in 1841. It was well known that he had intended to publish an account of his researches. It was known that plates had been prepared to illustrate his work; but the work, at his death, could not be found. I remember attending his sale in 1842, and amongst other things a lot of his old MS. sermons were sold, and other odd papers, some in MS. They were bought by a tailor at Torquay, who had a love of geology, and who kept those papers by him, intending to ransack them some day, to see what they contained. At length, to his astonishment, there was the missing MS., not fit for the press, written and re-written, improved and corrected here and there, and written in duplicate. They passed into the hands of a gentleman who, visiting at Torquay, bought things at the tailor's sale; and ultimately they passed into the hands of Mr. Vivian, long connected with cavern researches in Devonshire, and who in 1859 published a compilation from them. In 1869, the MS. having come into the possession of the Torquay Natural History Society, of which I happened to be the Honorary Secretary, I thought it a wise thing to publish the whole exactly as it stood; that is to say, the gaps, peculiarities of spelling, use of capitals, punctuation, and what not, stand exactly as he left them; and now the whole of those MSS. are before the world. I have thought it fair to this great and one of the earliest workers in caverns, who did not live long enough to enjoy the reputation which he would enjoy did he live now—I have thought it due to him to give you those particulars before entering on the researches which have been carried on more recently. The animals which Mr. MacEnery found are the following: elephant, rhinoceros, horse (more than one species), ox (at least two species), Irish elk (so called), red deer (two species), another deer, smaller than the red; reindeer, hyæna, tiger (which should be lion), wolf, fox, bat, weasel, lagomys (an animal, as Mr. Boyd Dawkins behind

me will tell you, now found in Siberia), mole, land-rat, water-rat, voles, bear (three species), and an animal which was called at that time *Ursus cultridens*, but which we now call *Machairodus latidens*. Not only did he find these animals, but he also found remains of human industrial art inosculating with the remains of the extinct animals; that is to say, he found flint implements in the floor of stalagmite and in the cave-earth below. Now, my friend in the corner reminds me that I have not told him what "stalagmite" is. Will you bear with me while I do so? So far as I am aware, all bone caverns occur in limestone rock. There are caverns in other rocks, but they are less numerous than in limestone rocks. The fact is easily intelligible to those who have paid attention to this question. Water passing through the limestone roof of the cavern, either by crevices or what not, on account of the carbonic acid which the water contains—which carbonic acid is derived from the atmosphere and from the decomposing vegetables on the surface—in virtue of that carbonic acid dissolves the limestone overhead. Having got through the roof the water drops, drop after drop: as you stand in the cavern you hear it going on. A portion only of the dissolved limestone is precipitated on the floor, and spreads from the point from which the drop falls right and left, until that spreading from another such centre, and from a third and fourth, meet it, and they form one continuous sheet. That limy matter formed as a floor is called "stalagmite." A portion of this limy matter is retained on the roof, and hangs dependent from it. If it be dependent from the roof and not formed on the floor, though made of the same material, it is called "stalactite." That is for my friend in the corner.

I have now to give you in MacEnery's own words—for I think this important—his description of his discovery of these flint implements, not his first description, but that which is most graphic: "Having cleared away on all sides the loose mould and all suspicious appearances, I dug under the regular crust [of stalagmite], and flints [meaning flint implements] presented themselves to my hand. This electrified me." Now ladies and gentlemen, carry yourselves back to the time when this was done. The man had been almost shaken out of his propriety by discovering the remains of extinct animals, some representing arctic climes, others tropical climes, and some temperate climes, all commingled in this cavern. And if that were not enough to shake the poor fellow's mind, he now discovers evidence of human existence at that same time. "This," he says, "electrified me: I called the attention of my fellow labourers, and in their presence

extracted from the red marl arrow and lance heads. I instantly proceeded to the excavation inside, which was only a few feet distant, in the same continuous line. The crust [stalagmite] was about two feet thick, the clay [cave-earth] rather a light red. About three inches below the crust, the tooth of an ox met my eye, and I called the people to witness the fact; and not knowing the chance of finding flints, I then proceeded to dig under it, and at about a foot I dug out a flint arrow-head. This confirmation, I confess it, startled me. I dug again, and, behold, a second of the same size and black colour. [Usually they are white.] I struck my hammer into the earth a third time, and a third, but white arrow-head, answered to the blow." I omit certain passages which it is not necessary to quote to you. He goes on: "Dr. Buckland [to whom he submitted all his discoveries] is inclined to attribute these flints to a more modern date, by supposing that the ancient Britons had scooped out ovens in the stalagmite, and that through them the knives got admission to the diluvium [cave-earth.] * * * Without stopping to dwell on the difficulty of ripping up a solid floor which, notwithstanding the advantages of undermining and the exposure of its edges, still defies all our efforts, though commanding the apparatus of the quarry, I am bold to say that in no instance have I discovered evidence of breaches or ovens in the floor, but one continuous plate of stalagmite diffused uniformly over the loam."

This, ladies and gentlemen, has always appeared to me to be one of the most instructive cases in the whole of this great question. Far be it from me to introduce anything theological in a scientific lecture, or any theology in this place. I have nothing to do with that; I have to do with science, and science only. But it must be borne in mind that here was an excellently good man, a priest of a Church that does not contend for the right of private judgment, exercising his private judgment: and the strong hand of authority, in the shape of Dr. Buckland, a priest of a Church that does contend for the right of private judgment, suppressing that man; or, but for that, fifty years would have been saved in this great question. MacEnery, it is clear from his book, was prepared to go the length on this question that Sir Charles Lyell and those who think with him go now.

In 1840, Mr. Godwin-Austen, a geologist I am happy to say still spared to us, and who, I believe, has done more for the geology of Devonshire than any man living, wrote a paper on the Caverns of Devonshire, and speaking of Kent's Cavern, made this utterance: "Human remains and works of art, such as arrow-heads and knives

of flint, occur in all parts of the cave, and throughout the entire thickness of the clay; and no distinction founded on distribution or relative position, can be observed, whereby the human can be separated from the other reliquiæ." So far Godwin-Austen.

In 1846, the Torquay Natural History Society appointed a sub-committee to spend £20 (they were poor, and could afford no more) in exploring a certain small portion of Kent's Cavern. I had the honour to serve on that sub-committee in 1846. From that time I have been more or less actively employed in cavern researches, which is now six and twenty years. Well, we discovered just precisely the same things as MacEnery and Godwin-Austen reported. The sub-committee consisted of Dr. Battersby, Mr. Vivian, and myself. Mr. Vivian drew up a report, which was read to the British Association in 1847, and to the Geological Society the same year. From his report, published in the British Association Transactions, I take the following passage: "The important point that we have established is that relics of human art are found under the unbroken floor of stalagmite. After taking every precaution, by sweeping the surface and examining most minutely whether there were any traces of the floor having been previously disturbed, we broke through the solid stalagmite in three different parts of the cavern, and in each instance found flint knives. * * In the spot where the most highly-finished specimen was found the passage was so low, that it was extremely difficult, with quarrymen's tools and good workmen, to break through the crust; and the supposition that it had been previously disturbed is impossible."

That is Vivian's statement, with which the other members of the sub-committee concurred. Now I have put before you the literature of this subject, and the question is asked, How did the scientific world receive these statements? Receive them! They utterly rejected them. Mark, I do not say the world, but the *scientific* world. Men of science were not at that time prepared to receive the statements that were made time after time by competent persons. Their scepticism was based mainly on two points: First, they were sceptical about the commingling of the flint tools with the remains of the extinct animals. I told you awhile ago that the same paper that Vivian read to the British Association in 1847 was read to the Geological Society the same year. They took the following notice of it: I will read you every word. This is in the *Quarterly Journal* of the Geological Society: "On Kent's Cavern, near Torquay. By Edward Vivian, Esq. In this paper an account was given of some

recent researches in that cavern by a committee of the Torquay Natural History Society, during which the bones of various extinct animals were found in several situations." That is every word: not one word, not the most distant intimation, that any flint knives had been found. The scientific world was not at that time prepared to receive this statement. This will have further significance when I tell you that on the wrapper of the Journal there is the following announcement: "The editor of the *Quarterly Journal* is desired to make it known to the public that the authors alone are responsible for the facts and opinions contained in their respective papers." And yet they would not allow Vivian's facts to go before the world. Now I felt, as a Fellow of the Geological Society, a little vexed at this; but as a man desirous that the truth should be accurately ascertained, I am not quite sure that I am seriously vexed at it. Their objection was that the work had not been done in a sufficiently systematic manner to compel conviction; and until it had been so done they felt themselves at liberty to withhold their assent. You perceive the effect of that was to compel us, who have taken up the question more recently, to go about cavern researches in a more systematic manner than had been done hitherto. And now I think we can come before the world without the slightest possible chance of any one gainsaying our facts.

There was, however, another point. When I read the list of animals reported by MacEnery as having been found in Kent's Cavern, I mentioned the name of one, *Ursus cultridens*, which I said we now call *Machairodus latidens*, formerly supposed, as you will see from the name, to be a species of bear. It was doubted whether that animal had ever been found in this cavern; for it was a curious fact, that though animals of the same *genus* had been found on the continent of Europe, none had been found in any part of Britain, and Kent's Cavern deposit was known to be more modern than those spoken of on the continent of Europe, in which remains of the same *genus*, not the same *species*, had been found.

I remember my friend, the late Dr. Falconer, who was sceptical on this question, said—"Well, you know, MacEnery went, as most priests do, to Italy, and in the Val d'Arno *machairodus* have been found. He, in all probability, obtained some of those remains, being interested in such questions, and brought them to Torquay, where they got mixed up with Kent's Cavern remains, and he, in perfect good faith, supposed the whole belonged to Kent's Cavern, whereas those remains of *Ursus cultridens* do not belong to Kent's Cavern at all." That was Falconer's hypothetical explanation.

You shall see whether we have succeeded, whether we have proved that MacEnery was correct on this point. In 1858 a new cavern was discovered at Brixham, on the opposite side of Torbay. That cavern almost as soon as discovered passed into my hands. A lease of it was taken; we ejected the proprietor; purchased the right to explore it for three years; and it was explored under the sanction of the Royal and Geological Societies of London, by a committee appointed by the latter society, and, being the only resident member of that committee, the work was entrusted entirely to my superintendence. The result was, we set to work in a manner that would compel the recognition of all such facts as might be discovered. And we again discovered precisely such facts in Brixham as had been reported again and again from Kent's Cavern. This helped to revolutionise public opinion on the question of the antiquity of man. In 1864 it was felt, during the meeting of the British Association at Bath, to be desirable to appoint a committee for the purpose of systematically and carefully exploring such parts of Kent's Cavern as remained intact. That committee is still in existence, and consists of Sir Charles Lyell, Professor Phillips, Sir John Lubbock, Mr. John Evans, Mr. Vivian, Mr. Busk, Mr. Boyd Dawkins, Mr. A. Sandford, and your humble servant, the Honorary Secretary. We have been at work in that cavern every day, without interruption, from the 28th March, 1865, to the present time, and the researches are still in progress. From that time till now, except in rare instances, when away from home, I have visited Kent's Cavern daily to see what the men had done, superintending their work, making notes of all the facts discovered. A monthly report is sent to our chairman, Sir Charles Lyell, and an annual report to the Association. I think, therefore, I may speak with some confidence of the facts I shall now bring before you. Will you pardon a little bit of boasting on my part? I have spent now, for nearly eight years, on an average, five hours a day in that cavern work; and that, ladies and gentlemen, is a sample of the manner in which scientific work is done in England—the pleasure of the work is the payment, and the only payment we receive. You perceive that the great objects we had in view were, first, to determine whether or not it was a fact that human industrial remains were found inosculating with the remains of extinct animals in that cavern; and, secondly, whether *Machairodus latidens* did occur there. I must now tell you a little about the cavern itself.

[The lecturer here referred his audience to coloured sections of

the parts of the cavern that have been excavated, on a scale of one inch to five feet.]

With reference to the deposits in the cavern. First or uppermost we found huge blocks of limestone covering in every direction the entire chamber which we first entered. Some of these blocks were a hundred tons in weight, some were only a few pounds, and they were frequently cemented together with stalagmitic matter. Beneath and between these blocks was a black material, which we call *black mould*, consisting of vegetable debris to a large extent, and which covered the cavern in every direction to the depth of three inches to a foot or more. Below that was the stalagmite, varying in thickness from an inch to upwards of five feet, but on an average from sixteen to twenty inches thick. In one particular part of the cavern there was under this floor a layer called the *black band* covering a space of one hundred square feet, and consisting mainly of charcoal. Below that we have what we call the "cave-earth," which we excavated to a depth of four feet. It is a light red loam, and with it there are mixed up about fifty per cent of angular pieces of limestone. That is the general succession of deposits. But when we came to certain parts of the cavern we found something else. With the same facts as before—the black mould, the stalagmite, and cave-earth—we found a lower stalagmite, and underneath it another deposit called the "Breccia." Allow me to recapitulate: in descending order, and omitting the blocks of limestone, we had first or uppermost, black mould; secondly, stalagmite of a granular character; thirdly, cave-earth, including the black band; fourthly, another stalagmite which is crystalline; fifthly, breccia—that is a cave-earth of a higher antiquity. I must next tell you how we explored; for I cannot conceal from myself that we were engaged in researches likely to revolutionise opinions that are considered grave; and I think that when men are so engaged they should pursue their investigations in a spirit of religious regard (mind, I don't say theological) for truth and accuracy. I trust it is in that spirit that our work has been done. I must show you how we proceeded. We set up at the doorway or entrance a line having a definite direction from the entrance to the back of the chamber. That line is defined just as rigorously as if you were laying down the datum line for a railway. We have got in, we will suppose, some ten feet from the entrance, and have cut a vertical section down through the deposit. Here is our datum line through the length of the cavern. We then fix a line at right angles to our datum line and a foot further in, so that we have a slip or

"parallel" a foot in width, and as long as the cavern is wide. We first take off the black mould carefully, examine it *in situ* by candlelight, bring it to the door and search it carefully, take out every object, no matter how small; everything that is not limestone is preserved, and put into a box; nothing else is put into that box. Then the material is thrown out of the barrow, and the workman gives it a last look to see that there is nothing more there. Then we proceed to the stalagmite below. We take that off in like manner, break it up to see what is in it, and put what is found into another box. Then we proceed with the cave-earth in the same manner, only more particularly. We always begin on the right of the datum line. The workmen have to take out a mass of earth three feet long, and one foot square in the section, in other words, three cubic feet, or, as it is called, a "yard." They examine that in the same way, and everything found is put into a box. Then they proceed with a second yard; everything there is put into another box. Then they proceed to the second foot-level in the same way, and so on to the fourth foot-level inclusive. Everything found during the day is sent to my house in the evening, with labels stating the date, the parallel or distance from the entrance, the level, the yard, &c. I wash all the specimens, number the labels, and write the same number on every object in that box. The whole are then carefully put away under lock and key. I have upwards of six thousand of those boxes in my house at this moment. If, however, it should happen, and it sometimes does, that a specimen drops out when a man is at work, and he does not see it fall, but finds it on the floor, though he could in a court of justice perhaps say, "I am absolutely certain it came from where I was working," still we insist that he shall be more particular than that, and that specimen is put in a box apart, and marked "uncertain." We eliminate all doubtful evidence. I thought it necessary to enter into these particulars in order that you may feel perfect confidence in the facts I have to state.

What did we find in the *black mould?* We found pebbles of various kinds, whetstones, angular and curvilineal plates of slate—probably intended as covers for earthenware vessels,—pieces of smelted copper, various kinds of combs made of bone. Fancy you have a bone about the size and shape of a shoe-lifter or shoe-horn, with teeth at the broad end; that would be like such combs as we found; but some of them were beautifully ornamented and some of them were rude. I fancied I could distinguish the comb that belonged to the "missus" and the comb that belonged to the servant-maid. Such

combs are well-known in the British Museum, and we know their ages archæologically. We found also spindle whorls, flint flakes, amber beads, charred wood. I said just now that we had no pecuniary reward. You must take that with a grain of salt, for we found a halfpenny of 1806 and a sixpence of 1846; but with those exceptions my statement is correct. We have found bones and teeth of men, pig, dog, fox, badger, brown bear, short-fronted ox, red deer, sheep, goat, hare, rabbit, water-rat, seal, birds, and fish; shells of snails, limpets, whelks, oysters, cockles, mussels, pectens, solens, and cuttle fish, and hazel nuts. You will observe from the list I have read there are no remains of extinct animals in the *black mould*. If you saw the objects you would know that they take us back to Romano-British and pre-Roman times. We have gone back some two thousand years at least before we touch the stalagmite; and we insist that whatever is in the stalagmite is older than the oldest thing in the black deposit lying on it. In the uppermost, or *granular stalagmite*, we found stones of various kinds, shells of cockles and cuttle fish, impressions of ferns, charcoal, bones and teeth of bear, elephant, hyæna, rhinoceros, horse, fox, and man, with flakes and cores of flint. You perceive we are now in presence of extinct animals. In the *black band* we found three hundred and sixty-six flint tools, flakes, and cores, a bone awl, a bone harpoon, a bone needle having a well-formed eye, burnt bones, remains of ox, deer, horse, badger, bear, fox, rhinoceros, and hyæna. In the *cave-earth* we found stones of distant derivation, such as pieces of granite from Dartmoor, whetstones, hammer stones, lanceolate and ovate flint tools, and flint flakes, a bone pin, two bone harpoons, charcoal, burnt bones, coprolites of hyæna, and teeth and bones of various animals. The bone tools have been found down as low as we have found the flint tools. There are people who have the power of disbelieving—I almost envy them—that the flint tools were made by men. I make them a present of them—metaphorically. But, I ask, what would they make of that bone needle, which I found with my own hands? What do you make of those beautiful harpoons and that bone pin, found at the greatest depth to which we have gone? They are unmistakable proofs of human existence, whatever you say about flint implements. Mixed up with these remains we have found the cave lion, another felis (the size of the lynx), the wild cat, cave hyæna, wolf, fox, two other varieties of fox, the glutton, badger, cave bear, grizzly bear, brown bear, mammoth, rhinoceros, horse, urus or wild bull, bison, "Irish elk," red deer, reindeer,

hare, pika or lagomys, water-vole, field-vole, bank-vole, *Arvicola gulielmi*, beaver, and last though not least, *Machairodus latidens*. MacEnery was perfectly right when he said that he found the teeth of that animal in the cave-earth, for we found them there too. I ought to correct myself. I said we have found *them* there; we have only found *one*. And now for my friend in the corner. My dear boy, if you commence scientific work do not suppose that your work is completed until it is done. Don't trust to negative evidence alone. We had been at work in Kent's Cavern seven years and four months, turning up and examining thousands and hundreds of thousands of bones and teeth of various animals, and all that time we had not found *Machairodus latidens*. Those who were sceptical about those remains having been found there had this additional argument at that time—that seven years and four months' labour had been expended in Kent's Cavern in the most systematic way, and yet *machairodus* had not been found. Yet towards the end of July last, whilst washing the teeth brought from Kent's Cavern, to my delight I found that serrated margin along the crown of the incisor which clearly established that I had *machairodus* in my hands. Ladies and gentlemen, I was delighted beyond measure, not only to have found this fact, but to have established the veracity of that grand old fellow, MacEnery.

In the *crystalline stalagmite*, that is to say, in the lower stalagmite, which is in some places twelve feet thick, we found the remains of bear only. Let me tell you again, my friend in the corner, we had found this old stalagmite in pieces such as you see represented here, incorporated in the cave-earth, and had gone on breaking them up, hoping to find remains of animals; day by day we had broken them up by hundreds: for eighteen months we had been doing that and never found the least trace of an animal. At last one day a piece was broken, and out came a bone. The negative evidence of eighteen months shrivelled up in the presence of that one specimen. After that time we found in this deposit bones in any number, but nothing but bear. There was no kind of indication of hyæna.

In the *Breccia*, or oldest deposit we have reached, we found an immense number of teeth and bones, all of them those of bear; and with them three undoubted flint implements.

I come now to the distribution of the teeth, and shall confine myself to a particular part of the cavern, termed Smerdon's passage, after one of the excavators. Though our principal workman, he can barely write the labels for me, but he is a noble fellow. I have such curious notions about education,

that so far as cavern researches are concerned I hold that man to be highly educated. In this part of the cavern we found several thousand teeth, and they were distributed in the following order: In every thousand there were 335 (about one tooth in every three) belonging to the hyæna; 295 per thousand horse; 161 rhinoceros; and they always occur in about that proportion—the hyæna always leading the way, the horse following next, and the rhinoceros next. These different species have not all the same number of teeth in their heads remember, and therefore the figures do not represent a corresponding ratio of the animals. The Irish elk 55 per 1,000, ox 35, deer 27, badger 22, mammoth 20, bear 18, fox 12, lion 6, reindeer 5, wolf 4, rabbit only one. That is the general relative prevalence of the teeth.

Now we come to the question of the condition of the bones. I see the time is going on. ("Go on.") I am obliged to you, ladies and gentlemen, but I have a throat, and it is not very strong; I will go on, however, for some time. The bones we found without anything like an approach to anatomical arrangement. We did not find the parts of a leg, for instance, lying together. So far as my memory serves me, only three bones in any instance, and that almost a solitary one, were found in their proper anatomical relation. Instead of that we found the bones of different kinds of animals lying together, without any order whatsoever. They were most of them in such a condition as to stick to the tongue. If applied in this way (pardon the trick—not a very elegant one) they adhere, and some of them sufficiently to support their own weight. That is the general character of cavern bones—they have lost their gelatine. Many of them were invested with stalagmitic films; in whatever level we found them they were frequently thus covered with stalagmite. Supposing, for instance, I found this bone a couple of feet deep. I remove the earth and find it is covered with stalagmitic matter. The inference is clear—the place where the bone was found was for a time the upper surface, and the water dripping from the roof invested it with this stalagmite; and inch by inch, as you ascend from the bottom, that fact presents itself. Some of them are entire, not broken or marked in any way. The only information they give is that they are the bones of certain animals. There was in some cases this special feature, that we found bones crushed under the blocks of stone, though the parts were all lying in their places, showing clearly enough that the bone was not crushed before that block of limestone fell on it from the roof, and the deposit below being firm and capable of offering resistance, the specimen was broken

in situ. That is constantly happening. Then some of them are fractured. For instance, here is the lower end of the leg bone of a horse: you perceive the form in which it is broken. I went one day to the Zoological Gardens, and said to the man who has charge of the hyænas, "Will you show me your most powerful hyæna?" He did so. "Give him a shin bone, will you?" And I saw how "Jack" took the shin bone: he did not try to break it this way, by taking it between his jaws in the line of their length, but took it in across his mouth—I am not an hyæna, and cannot exactly manage it—but he took it nearer to one end than the other, placed the remote end on the floor, and then, pressing on it with his foot, struggling, and heaving and wrenching, he broke it in that way. I saw Jack try his jaws on several shin bones, and I cribbed from him such pieces as I could: there is what I saw Jack do, and there is the bone we found in Kent's Cavern. Are they not as like as two peas? I said to the keeper of the hyænas, "So far as is consistent with your duty, will you give Jack any number of shin bones, and at the end of the week I will come again. Crib from him as many pieces as you can." When I returned I found a nice little basket of bones, and all, excepting the splintered bits, were of precisely the shape of those we find in the cavern. Here then is an indication of the hyæna's work. Bear in mind that the bone breaks in this way not because of Jack's teeth alone, but because of the structure of the bone itself. But there are certain bones such as that I hold in my hand now. And here I shall differ from some of my friends, who know that I am heretical on this point. I have a speculation about this bone, which you see is split longitudinally, and I know that Jack cannot so split it. I am perfectly willing to admit that the hyænas of old were more powerful than the hyænas now; but I know that neither the lion nor the tiger in the Zoological Gardens can split a bone. The keepers told me that in order that these creatures may get at the marrow, the butchers are instructed to split the bones for them. The animals cannot split the bones, and yet we find that a great number of these bones are split. My conclusion is that they were split by man. And then I am met by this difficulty—How do you suppose it possible for savage men, having no better tool than that flint implement, to split a bone? I have thought a good deal about the question. I do not tell you that they were split in this way; but how a savage might succeed in splitting them. You see the distinction. Thinking it not very difficult to descend back to the savage condition experimentally, I tried the experiment in this

way: All the bones that are split are divested of their articulating surfaces—the ends are gone. It appeared to me that that was a preliminary step in the work. I therefore took the first large stone that came to hand as an extemporised anvil. I placed the bone with the end projecting over the anvil, and with another stone for a hammer, dealt it a blow such as I presume a savage could have dealt it, and broke off an end, and then broke off the other end. And now I saw that there was no possible reason for a man to split the bone in order to get the marrow, for he could get it out with the first small stick that came to hand. I do not believe that bones were split for that purpose. What next? I broke off a small bough of a tree, resolved not to use any modern tools, and inserted the smaller end into the marrow cavity, and then using it as the paviors do their rammers, drove it into the bone, and split it into these pieces; and I could do it again and again a thousand times; that is a method by which the savage, with no better tools than the cave-men possessed, could split them; and I believe they split them not to get the marrow, but to get bone implements, which they did make. I must tell you again that very few of my friends agree with me in this matter. But understand, I only say that men could so have split them. As some of these split bones have the marks of teeth on them, my friends say that the hyænas split them. Well, I say the hyæna's jaws are not powerful enough. If you make a vice in the shape of the hyæna's jaws you cannot split this bone; you may fracture it, but not split it longitudinally. I believe that men split the bones and threw some of them away, and the hyænas gnawed them after. It would seem that accident was so kind as to furnish me with an illustration; for whilst at my savage work, a gentleman calling on me brought with him a huge dog, which ran off with one of the bones. I shouted to him to save my bone, whatever became of the dog. On recovering it I found it gnawed charmingly. Some persons tell me that this idea reminds them of the "Happy Family"—man and the hyæna living together. Now I do not apprehend that there was anything of the kind. Man occupied Kent's Cavern. There is the place where he made his fire; that was his home more especially. Man was then a hunter, a fisher, not a cultivator of the soil. A savage in such a climate as ours would have to migrate from one cavern home to another, and when he left one cavern the hyænas would take possession of it, to be ejected on the return of the human occupants. The hyæna gives way to man. Humboldt tells us that the same thing occurs with the jaguar in South America; and my

friend the Rev. Canon Tristam tells me that it occurs with the hyæna in Syria and Morocco. I don't think it will be necessary for me to detain you much longer, but there is just one point that I wish to illustrate. I hold that this, the area of the black band, was the part of the cavern which man regarded as his dwelling-place more especially; first, because it was near the entrance and the light was available. I know that to be the case, from having sat down there and, without artificial light, read and written letters. And secondly, because it is a dry part of the cavern. A friend of mine said—"My good fellow, if they made a fire there they would smoke themselves to death." I thought I had seen smoky huts in the Highlands, and that men did live in the smoke; and I have often been in a railway-carriage when every man has been smoking his cigar, and I have lived through it. But to put this to the test, the workmen got six faggots of wood, and we piled them up and fired them. The temperature of Kent's Cavern is never below 52 degrees, so the inhabitants would not want much artificial heat. We made a huge bonfire, reminding us of our youthful 5th of November days. We sat around and saw it burn out, and we were not in the least degree inconvenienced by the smoke. There is nothing, my young friend in the corner, like an experiment to test all questions of this kind. I shall not say more about this subject.

Coming to the question of time, we have gone back some two thousand years at least—that is the minimum, it may be more—before we get through the *black mould.* We enter then the granular stalagmite, and we know from the nature of the case that that thickness of stalagmite must indicate an enormous length of time, inasmuch as the stalagmitic floor cannot be formed faster than the limestone is dissolved overhead, and the solution of that limestone is due to the presence of carbonic acid, and there is no possibility, under existing conditions, of any other water entering that cavern than what falls on the hill as *rain.* I do not ask you to take the thickness of the stalagmite as a chronometer, but will tell you a fact. There is in one part of the cavern a huge boss of stalagmite rising up from the floor. That boss betokens that its formation was comparatively very rapid. Take that rapid rate as the measure. There is on the boss an inscription—"Robert Hedges, of Ireland, Feb. 20, 1688." For 184 years the drip has been going on, and it has failed to obliterate that inscription; the film of stalagmite which has accreted on it is not more than the 20th of an inch in thickness—nearly 200 years for the 20th of an inch, and you have

five feet to account for! But whatever may have been the time necessary for the formation of the stalagmite, the cave-earth is older still. There is another and more ancient stalagmite thicker still; below that there is another deposit older than all, and in that we found human implements. These are the facts so far as they go. But I fancy my friend in the corner is suggesting to me at this moment that possibly that inscription is a forgery, and is not as old as it appears. Very well put, my boy, and unless it is answered the argument is not worth much. I have MacEnery's MS. in my keeping, as secretary of the Torquay Natural History Society. He described it in 1825. I have it in his handwriting. The inscription is good then for forty-seven years. Further, it was not newly cut then, for he says the letters are glazed over with a film of stalagmite; in short, his description applies accurately to it now. We have, therefore, the best possible reason for believing that it is genuine in every sense.

And now, having simply contented myself with stating the facts, there is little I need give you in the way of inference. You are as capable of drawing the inferences as I am. I have thrown the seeds into your minds; let them germinate; and I will only say, as a parting word to my young friend—Be careful in scientific inquiries that you get a sufficient number of perfectly trustworthy facts, that you interpret them with the aid of a rigorous logic, that on suitable occasions you have courage enough to avow your convictions, and don't be impatient or annoyed if your friends don't receive all your conclusions, or even if they call you hard names.

A

FRAGMENT OF FARADAY'S ELECTRICAL DISCOVERIES.

A LECTURE

BY

W. F. BARRETT, Esq.,

Delivered in the Hulme Town Hall, Manchester, January 22, 1873.

I AM glad to think the name of Faraday is familiar to you all, and that this large audience has been brought together by the desire to know something more of what he did. My purpose to-night is not so much to speak of Faraday himself as to show you a few of his discoveries, and let them speak for me. Of the beautiful inner life of Faraday I shall say nothing; that you have already heard from abler lips than mine; and those of you who had the misfortune not to hear Dr. Gladstone's lecture will, I hope, read the printed report, or the delightful memoir Dr. Gladstone has published. Let me only remind you that it was chiefly by a passionate devotion to nature that Faraday lifted himself from obscurity to be one of the greatest, if not *the* greatest, of experimental philosophers England has ever produced.

And now let us ask what especially made Faraday so famous? It was mainly his wonderful series of electrical researches—the outcome of his patient skilful labour for a quarter of a century. These bulky volumes which lately fell into my possession contain the record of his researches as they appeared in the Transactions of the Royal Society. Some present may like to look at these books after the lecture, as they happen to have a special interest of their own. You will see the neat handwriting of Faraday inscribing each of his papers to his early fellow-worker and life-long friend, Mr. Richard Phillips. The critical comments by Mr Phillips fill the margin, and are interspersed with occasional pencil memoranda by Faraday himself.

Now it is very evident that I can give you no adequate idea of these twenty-five years' work in a single hour, so I must confine myself this evening to one portion of his work; and I will select that which chiefly related to *magnetism*, and the association of magnetism with other forces. Let us first of all recall a few of the familiar facts of magnetism; we shall thus be better able to understand the significance of those facts that Faraday has revealed. Here is an ordinary piece of steel—it is a carpenter's chisel. If I dip it in iron filings you see no filings adhere, for the steel has no more influence on the filings than a piece of stone would have. But here is the thing we call a magnet, over one end of which I now draw the steel one way—thus, and again dip the chisel in the iron filings. Now you observe what clusters of filings adhere to it. Evidently something has been given to this steel that it did not possess before; that something is what we term magnetism, and the chisel is now a perfect magnet. If I suspend the chisel one end turns, as you see, towards the north; it behaves, in fact, precisely like this compass needle. The eye cannot distinguish between the magnetism of one end of the chisel from that of the other, yet it is evident there is a profound difference in the behaviour of the two ends towards the earth. Let us mark the extremities. For the sake of distinction I will put this disc of red paper on the end that points to the north, and this disc of blue paper on the end that points to the south, and similarly here also on this other magnet. I will now bring one of these marked ends towards one end of the suspended magnet. You notice that the two blue poles repel each other, and the two red poles repel each other. They are unfriendly poles; whereas the blue and the red, or the red and the blue, are friendly poles; for you see they attract each other. In other words, the *unlike* ends of a magnet have the power of attracting each other; and the *like* ends have the power of repelling each other. But unmagnetised steel or iron is attracted to either end. Rolling the magnet in iron filings, you see how thickly they cluster at the extremities, whilst none are attracted along the central portion. These regions of power at the two ends are termed the *poles* of the magnet, and we have already learnt how to recognise each of these poles by the law which expresses their action on each other.

To one more elementary fact I would just draw your attention. The power emitted from the poles of the magnet not only passes through space, and can influence distant bodies; but, further, this wonderful magnetic influence is not interfered with by the presence of opaque or dense substances. Here, for instance, is a large

piece of wood, which my assistant will hold between the magnet and the compass needle. You observe the magnet drives the needle aside, even though this opaque obstacle intervenes. The magnetic power is not screened off, nor is its nature altered, for on removing the wood you find there is a blue pole attracting the red, as before. If I shut the magnet up in a box, or seal it hermetically in this glass tube, still the power passes through the box or the glass. Not only does the magnetic influence pass through these dense substances, but it passes through them without in any way being lessened by their presence. Allowing the needle to swing to and fro in front of the magnet, you notice the rate of its vibration (by means of which magnetic force can be estimated) is not changed by the introduction of, say this big book, between the end of the magnet and the needle. Nor is the interposed obstacle, if not a magnetic body, like iron, in any way affected by the transmission of this power. For example, we will let the human body intervene. On one side of my assistant's head I place our needle, and now on the other side I hold the magnet. Though the magnet is entirely screened from view, yet, observe, the needle is driven aside. Here the magnetic action has passed through the skull and the delicate tissues of the brain, and, as Mr. Williams could tell you, without the least sensation being experienced. How it has been transmitted, or by what medium the force is conveyed, I cannot tell you, nor has anyone as yet solved this wonderful enigma. Some ten years ago, in the laboratory of the Royal Institution, I was once checking the motion of a magnetic needle, suspended within a glass shade, by means of a magnet held at some distance. Mr. Faraday happened to be in the room at the time, and I well remember his words as he turned to me and said: "How mysterious is that power you have there; the more I brood over it the less I seem to know." Yet I suppose no one in this world knew more of it than he.

When we thus stand upon the brink of the unknown it is useful and helpful to aid ourselves by the imagination. Faraday thus pictured to himself the magnetic power streaming from the poles of the magnet, and spreading through space in directions which he called the "lines of force." Somewhat as rays of heat issue on all sides from a hot body, so magnetic power radiates from the two poles of a magnet. You can feel the presence of the heat rays, but you cannot feel the presence of these magnetic rays. Is there any way by which we can make this radiant magnetism visible? There is a simple way. It is by means of strewing iron

filings around the magnet. By tapping the surface on which they rest we loosen the adhesion of the filings to the table, we overcome their *stiction*, as it were, and they will then arrange themselves in the direction that the magnetic influence gives to them. And this experiment was one which Faraday was never tired of repeating. His "lines of force" are generally known by the name of magnetic curves. Now it would be impossible for me to show this audience an experiment of this kind upon the table, as it is above you; but I think we may devise a plan by which you will all be able to see the direction of these lines of force. In this way I will endeavour to make these curves visible. Here is a tiny magnet fixed to a glass plate, the other side of which has been covered over with weak gum-water, and is now dry. I will strew fine iron filings over the gummed plate, and by tapping it I obtain the curves. Now I will breathe upon the plate, the gum is thereby moistened, and the filings now firmly adhere in the direction they were able to assume on the smooth dry surface. Mr. Armstrong will now be kind enough to put this glass plate as a slide into the lantern, and there you behold upon the screen those exquisite magnetic curves. You notice those straight lines passing from pole to pole of the magnet. You notice also those splendid curves sweeping through the wide space above, and clasping each other, as it were, in their friendly arms, for those two black ends are the friendly poles of a magnet, and every fragment of iron has thereby been endowed with friendly poles juxtaposed. Here is another glass plate, on which two similar, that is, unfriendly poles are placed side by side. My assistant has prepared the plate ready to put in the lantern. You now see how different is the appearance—how conflicting those lines of force appear: no longer linked harmoniously together, they now, in seeming hatred, turn from each other's influence. Midway between the magnets their mutual influence appears to meet, and you observe what a barrier seems to be there. Every fragment of iron along that line of separation has two like poles juxtaposed; hence the repulsion that occurs. We might vary this lovely experiment in many ways. Here are two little semicircular magnets fixed on the glass, and most instructive is the disposition of the curves as we change the relative position of the little magnets. Here they are with convex sides adjacent, and here the opposite way; here, again, one is placed at right angles to the other, and here are the curves when a piece of iron is fixed near one pole. In recent times a high degree of significance has been given to these curves by the labours of eminent mathematicians. And as the existence and

direction of the "lines of force" help our conceptions of how action can occur at a distance, so their matchless symmetry excites our admiration and our wonder. I hope many present will repeat this experiment at home; it is within the power of the youngest lad to do so. A sixpenny horseshoe magnet can easily be procured, so can a few iron filings, and a piece of muslin from which to shake them; then a pane of window glass with white paper below, or a sheet of cardboard on which to scatter the filings, and you have all you need. You can magnetise a knitting needle or a knife, and try the curves from them as well as from the horseshoe; remembering always to place the magnet beneath, and to tap the surface on which you have scattered the filings.

Instead of tossing up the fragments of iron by tapping, I will now suspend a little rod of iron by a thread; obviously it will place itself in the direction of the lines of force at the place where it is suspended. Here is a magnet with its poles turned upwards, and between the poles a nail is hanging: the motion of the nail is made visible to you by the straw index attached to it. The nail, attracted by both poles, sets itself, you see, along the straight lines of force that stretch from pole to pole, along the "magnetic axis," as this direction is termed. Now you will understand the meaning of one of the great discoveries that Faraday made. He found that on hanging between the poles of a magnet a peculiar kind of "heavy glass," that he had made in his early years, it was not attracted to either pole of the magnet, but was repelled from both poles, and, being repelled from both extremities, it necessarily placed itself across those lines of force that you saw just now upon the screen. Likewise, he examined many other substances, and found that almost everything upon which he could place his hand was repelled by a strong magnet; but, of all substances, the metal bismuth was the most strongly repelled. The repulsion of bismuth, however, is upwards of a million times less powerful than the attraction of iron to a magnet. Hence, this discovery had escaped attention before, if we except one or two observers, who had previously recorded the fact that bismuth had some little action upon a magnet.

It is necessary, in order to exhibit this repulsion on bismuth, to have an exceedingly strong magnet; and, for this purpose, ordinary magnets are not so well adapted. It is, however, easy to construct one sufficiently powerful, by the passage of an electric current round soft iron. Here, for example, is an ordinary poker, and if I dip it in some nails, it does not at present attract any of those nails, simply because it is

an ordinary piece of iron. But, now I will encircle the poker by this coil of wire, through which an electric current is passing, and you observe how vigorously the nails are now attracted. If my assistant breaks contact with the battery, the nails instantly fall away; the magnetism endures only so long as the current passed through the wire which surrounded the poker. Instead of using a poker I will use these two larger bars of iron, and pass the current through the massive coils that surround them. We shall, of course, get a proportionately larger effect. See, the whole of this big dish of nails is whipped up; and what a beautiful chain the nails make as they depend from the ends of the magnet. How exquisite a grace is given to these uncouth things by the symmetry of arrangement and subtlety of curve that springs from the play of magnetism and gravity! We will stop the current: the magic has gone; the nails instantly fall off. So that when the electric current (which comes from a voltaic battery below the table) passes round these coils, it magnetises the iron: when the electric current ceases to pass, the iron ceases to be magnetised. This phenomenon is termed "electro-magnetism," and this instrument is an electro-magnet. It was not discovered by Faraday, but it was applied by him in some of his discoveries, and more especially in the one I have just alluded to, in which he discovered the repulsion of bismuth. I will now show you this repulsion. Here is a powerful magnet, kindly lent me from Owens College; and if Mr. Armstrong will cast a beam of light inside this framework which envelopes the poles, we shall be able, I think, to see what is going on inside. Here is a fine silk fibre, suspending at the lower extremity a little bar of the metal bismuth. I have attached to that little bar of bismuth a split straw, and at the end of the straw I have placed bits of white paper to make its motion visible. The whole is surrounded with a glass case to protect it from currents of air. Now I anticipate that when the voltaic electricity is allowed to convert the iron into a powerful magnet, the bismuth will turn and place itself across the lines of force, and thus come in a line with you at right angles to its present position. I will make the contact with the battery, and I hope what occurs will be visible. Watch the effect. The bismuth is marching round and arranging itself in the position just indicated. I now break the contact. The current having ceased to pass, the bismuth has now resumed its first position. Faraday termed those substances that place themselves across the lines of force "dia-magnetic;" and those substances that place themselves along the lines he called "para-magnetic." Iron is an example

of a para-magnetic substance; bismuth is an example of a dia-magnetic substance. Here is a list of substances arranged in the order of their magnetic power, the highest being first:—

PARA-MAGNETIC.

Iron.	Manganese.	Crown Glass.
Steel.	Chromium.	Platinum.
Nickel.	Titanium.	Oxygen.
Cobalt.	Palladium.	

DIA-MAGNETIC.

Bismuth.	Tin.	Silver.
Phosphorus.	Zinc.	Copper.
Antimony.	Flint Glass.	Gold.
Heavy Glass.	Mercury.	Organic Bodies.
Sulphur.	Lead.	Water.

Liquids and gases Faraday also examined, and found, for example, that the gas oxygen was slightly magnetic. The magnetic nature of oxygen he believed had an important influence on the small variations which a magnetic needle undergoes during various periods of the day; but this question of atmospheric magnetism, as Faraday termed it, has not yet had the full investigation it deserves.

Now, associated with the discovery of dia-magnetism, and, indeed, immediately preceding it, was a discovery which Faraday termed the "magnetisation of a ray of light." He tells us how he was led to this discovery. "It has been my long and constant persuasion," he remarks, "that all the forces of nature are mutually dependent, having one common origin." Just as the intrepid explorer of some unknown region is guided by a prevailing idea, so Faraday, through all his work, was fascinated by this dominant impression that the forces in nature were one in essence, though different in manifestation. And hence he hoped to find a bond of union between such different powers as gravity and electricity, or chemical action and electricity, or magnetism and electricity. In much of this he succeeded. And now he seeks to establish a connection between such utterly different powers as magnetism and light. To an ordinary mind such a task would seem both hopeless and foolhardy; but Faraday had not an ordinary mind, and to him the effort was full of hope. Not disheartened by repeated failures, he tries at last what is termed polarised light, and sends a ray of such light across various crystals placed between the poles of the magnet. Among other crystalline bodies he hits

upon the heavy glass to which I referred in speaking of diamagnetism. He finds that a ray of polarised light sent through this heavy glass is so influenced, that the light is obscured when the magnetism passes round the coils, and that the light is instantly restored when the magnetism ceases to pass. So that here he proves the connection between magnetism and light; and he called, as I said, this famous discovery, "the magnetisation of a ray of light." But it is more probable that it is the magnetisation of the glass itself; for he afterwards found that the glass was acted upon by the magnet. But, regarded in any way, it is a most famous discovery, and altogether unique in the history of science. Those famous words spoken by Macbeth here seem specially applicable to Faraday, and his spirit of courageous investigation:

> 'Tis much he dares;
> And to that dauntless temper of his mind
> He hath a wisdom that doth guide his valour
> To act in safety.

Now we must turn to quite another region of Faraday's discoveries, and I shall want, for this purpose, an instrument to show you the existence of very feeble electric currents; for Faraday succeeded in discovering two entirely new forms of electricity, one of them being evoked by magnetism. Very tiny effects he obtained at first, and I am anxious to show you some of these tiny effects, and then afterwards we shall rise up to the grander and more palpable results obtained in subsequent years. The ordinary telegraph is an instrument for detecting the presence and the direction of electric currents, and, if I explain to you the construction of the electric telegraph (as some have specially wished me to do), you will understand the means which I shall employ to render these feeble currents visible; and, therefore, I hope our time will not be wasted by this digression. Here is a copper wire stretched from end to end of the lecture table. I can join up this wire with the battery which is below the table, and you will find that the wire conveying the electric current, whatever be its nature, becomes magnetic. A magnetic needle tends to place itself across such a wire. Here is the needle, with which you are already familiar, placed below and parallel to the wire. Now my assistant will make contact with the battery, and you will observe that the moment the contact is made, and the current flows through the wire, the needle will place itself nearly at right angles to the wire. Make. There it goes! the red end moving towards you. Now I will cause the current to flow in the

opposite direction through the wire. Reverse. You now observe the blue end moves towards you. So that, when the current passes in one direction along the wire, one end of the needle moves across it; and when it passes in the opposite direction the other end of the needle moves across it. Now, supposing that we had a means of placing this needle under the influence of our electric current, conveyed to a point far away from the battery, still the same effect would occur. Moreover, if the current is caused to pass in one direction above the needle, and in an opposite direction below, it will conspire to produce an augmented deflection. This can be done by coiling the wire several times round the needle, and in this way we shall find the means of detecting comparatively feeble currents by coiling the wire a great many times, and by using a smaller and lighter needle. Here is a large wooden hoop, round which is coiled a quantity of wire. In the centre of this hoop a magnetic needle is hanging. If, now, we send the current round the hoop, you observe how strongly the needle is driven on one side. Now, supposing that we agreed beforehand that one motion of the needle on one side should signify the letter *a*. To communicate that letter I should simply make the contact for a movement once. Supposing I wished to communicate *b*, and we agreed that two movements should represent that letter, two contacts would impel the needle twice, three contacts thrice, which we will call *c*, and so on, using little stops at each side of the needle to prevent it going too far. So, by agreeing upon a table of signs—making the letter *a* one dash, *b* two, *c* three, *d* four, and *e*, by reversing the current, one in the opposite direction, we can, by properly combining these signs, convey all the letters of the alphabet; and, if we can convey the letters, we can convey the words which those letters go to make up.

But I need to show you the existence of very much feebler currents than those ordinarily employed in telegraphing. How can that be done? It can be done in one way by using a smaller needle and fastening a piece of looking-glass to it. Here, for instance, I will place this strip of looking-glass alongside the needle, thus. Now, whenever the little magnetic needle moves, the strip of looking-glass will move; and if a ray of light is thrown on the glass, as it now is, you see the reflected ray rapidly traversing the walls of the room over there. Just as a schoolboy twists a ray of light about by moving a bit of looking glass, so the moving needle twists this light about. So a very slight motion of the needle will reveal itself by a great motion of that ray of light. I will send a feeble current round the coil. You observe the

needle has scarcely moved, but the light has gone right round the room, so rapidly that your eye could hardly follow it. So that, if we use a long index of light, we may reveal a very small motion, and there is this great advantage—the index of light is entirely without weight. Now here is a delicate little instrument, in which the principles I have explained are applied for the purpose of detecting weak currents of electricity. It is a modification of what is known as Sir William Thomson's reflecting galvanometer. There is a drawing of its construction on the wall. It simply consists of a smaller coil of wire, within which is a very small magnet, hung up by a thread of unspun silk. Attached to the magnet is a light mirror, about the size of a shilling. From this lantern there issues a narrow but powerful beam of light; and I dare say I shall be able to cast that light upon the little mirror, which reflects it, as you see, on to this long scale. Now, when you see that spot of light moving, it will indicate the fact that the little magnetic needle within the instrument is being moved, and the movement of the needle indicates the existence of an electric current passing round the coil that encircles the needle. Hence, you will be able to reason to yourselves that, if the spot of light moves, it shows the passage of an electric current round the instrument. If the current moves in one way, the spot of light will go in one way; if the current moves the other way, the spot of light will go the other way.

This plan is very similar to the arrangement adopted for telegraphing from England to America. Owing to the long distance that intervenes, there is a considerable retardation and loss of the current, and hence very delicate instruments are necessary A code of signals is agreed upon previously by the electricians between England and America. Now, let me show you how we can telegraph by means of this instrument, and at the same time we shall be able to test its extreme delicacy. Here I have a piece of beef-steak, and I am about to cut it with a steel knife and plated fork, the conjunction of which in the juices of the meat will evoke an electric current. I will try to make the current evident, and I dare say we can telegraph by its means. I am now fastening the wires from the galvanometer on to the knife and fork. Now I plunge the fork into the meat. The spot of light, you observe, still remains unmoved at zero. To generate a current a second different metal is necessary. Watch the light as I draw the knife through the meat. You see it is driven right off the scale. Let me send a little word by this novel telegraph. I will simply cut the meat to produce the deflection. One, two, three

flashes have gone, and the light has come to rest; that is evidently the letter *c*, for you remember we agreed it should be so indicated. Again I cut the meat: one flash, that must be *a*. Another interval; and one, two flashes, that is *b*. The light now remains at rest, so that is the end of the word. What does it spell? *c a b*. So you see we have actually signalled for a cab.

In this way, then, we can detect the existence of a very feeble current. Now Faraday found that when an electric current passed along a wire it gave rise to the existence of another feeble but momentary current in a wire near it. I will connect the galvanometer with this flat coil of wire, and you will find that every time I bring the coil near another coil of wire through which a current is passing, we shall get a sudden wave of electricity produced, which will reveal itself by the movement of the spot of light upon the screen. Here, then, is one coil, and here is the other. Through the distant coil I now send a current, and though this wide space intervenes, which, indeed, we may fill by opaque objects, yet a sudden current is generated in the second coil attached to the galvanometer. Now I will break contact with the battery, and there is a sudden deflection in the opposite way. So that when we make contact, what is called the "induced" current is created in one direction; and when we break contact another induced current is created in the opposite direction. Both are only transient currents produced by what is called "induction." In modern times these currents have been exalted by the instrument known as the "induction coil." Here, for example, is such an instrument, where these transient waves of electricity are able actually to produce sparks of considerable strength, accompanied by a noisy discharge—a mimic thunderstorm in fact. When the discharge is caused to pass through a partially vacuous space it lengthens enormously. In front of the table is hanging a very long vacuum tube, and if the lights are put down you will now be able to see the beautiful luminous effect produced by the discharge passing through the tube.

I will not dwell longer on this, and only mention it in connection with a still more famous discovery—I think quite the most famous discovery—that Faraday made. Faraday showed that a *magnet* behaves like an electric current, and produces an electric wave by its approach to, or recession from, a coil of wire. Let me show you this. Here are our coils and our galvanometer as before. I wish you to notice that the mere approach of the magnet to the coil will cause a movement of the ray of light. That is to say, a wave of electricity has been

produced in the coil similar to that wave of electricity you saw produced just now from the current itself. These long wires reaching to the galvanometer enable me to go far away from the instrument, and thus remove the disturbing effect which the magnet might otherwise produce. Approaching the magnet to the coil you observe how the light rushes on one side. Note, it is only a transient disturbance—the needle quickly comes to rest again. I will now withdraw the magnet, and you observe that the light rushes in the contrary way. So that the magnet acts exactly like the electric current acted, producing a current in one way by its approach, and a current in the opposite way by its recession. I trust you will now understand clearly that the mere to-and-fro motion of a magnet to a coil of wire can give rise to a series of electric currents alternately in opposite directions in the coil of wire. These are termed magneto-electric currents.

Faraday, then, discovered magneto-electricity. But he went further. He reasoned that the earth itself is a great magnet, and that we ought to obtain electrical currents by merely moving a joined wire to and fro in certain directions anywhere on the earth. And this he proved to be the case. Here I have an arrangement by which I hope to make this evident to you. The instrument before me consists of a coil of wire, that can be turned round rapidly. By a simple contrivance, the alternate currents that will be generated when I turn the coil can be made to pass in one direction. Wires now join the instrument to the reflecting galvanometer. I will take the coil to the far end of the platform, that we may be clear of any magnets that happen to be on the table, and so be subject only to the magnetism of the earth. If then our spot of light moves when the circle turns, it shows that an electric current has been produced by the magnetism of the earth. Observe! the slightest motion of the circle produces a current. But is it not possible that we may have obtained a current from some other cause, or that the movement of the needle may have been due to the shaking of the floor? The pursuit of natural knowledge should teach us to be very cautious in all our conclusions. If this current be really due to the earth, then if I turn the circle in the opposite way it ought to produce a current in the opposite direction. I will do so. Before, the light went from me, now it comes towards me. I think, therefore, no doubt can now rest in any of your minds as to the existence of electric currents generated by terrestrial magnetism. If instead of this joined wire you clasp your hands, previously moistened, and merely sweep your arms to and fro, an electric current in like

manner will be generated, which, though insensible, will pass through your arms and chest.

Faraday's marvellously quick power of observation enabled him to discern the feeble effects he first obtained from these magneto currents. He was a true *seer*, and almost makes us believe what an eminent writer says—that "the greatest thing a human soul ever does in this world is to see something, and to tell what it saw in a plain way. Hundreds of people can talk for one who can think, but thousands can think for one who can see."

Some time after Faraday's early experiments, it was found possible considerably to augment the electric currents obtained from a magnet, and it was reserved to a fellow-townsman of your own, to a Manchester man, Mr. Wilde, to crown Faraday's discovery of magneto-electricity by the construction of an exceedingly powerful magneto-electric machine. By this machine the tiny effects that you have observed are much exalted, and electricity is produced in such abundance as to heat thick iron wire redhot, or to yield a brilliant light. Through the great kindness of Mr. Wilde, I have one of his instruments upon the table to-night. Here you see a number of magnets, and between the extremities of these magnets is a quantity of coiled wire. Now, when this handle turns round very swiftly, it will simply do in a mechanical way what I formerly did with my hand, namely, move the coiled wire to and fro. My assistant is now working the machine. You see that a considerable length of iron wire is actually glowing redhot. By shortening the wire, I think we shall be able even to melt it. [The wire was quickly fused.] One of the latest applications of magneto-electricity is for the purpose of lighthouse illumination. Thirty years after Faraday had made the discovery of magneto-electricity, that child of his, as it were, grew into a mighty power, and now shines like a midnight sun over the reefs that surround our coast. For powerful machines, similar to this, have been in use for some time for lighthouses on the south coast of England; and Mr. Faraday had the great joy of living to see this grand application of his discovery. It is not unlikely that at some future day the tidal power may be used to drive these lighthouse magneto machines, so that the very ocean itself may be made to warn mariners of its own fury.

There have been many other practical results to which magneto-electricity has been applied. Sir Charles Wheatstone, for instance, has constructed private telegraphs by the same means. If you can once produce a current of electricity, almost anything is possible. And here, in the dial telegraphs that I have on the

lower table—for which I am greatly indebted to the superintendent of the postal telegraphs of this district—we have currents of electricity produced in a similar way to that which you saw just now, namely, by the movement of coiled wire between the extremities of a magnet; so that by merely turning a little handle you can generate an electric current, and by a proper series of signs or dials you can signal any letter or word that you wish. Will the telegraph assistant kindly ring the bell by the passage of the magneto current? [The bell was rung.] He is now going to send a message from one end of the table to the other. The bell, we may suppose, has alarmed the person in charge at the far end: he goes to the instrument, turns off the switch, and awaits the message that is coming. If any of you after the lecture wish to look at the instruments, and see how the messages are transmitted, the assistant will be ready to show you. This means of telegraphing is very convenient, as it does away with batteries and acids, and substitutes the mere motion of the hand.

Magneto machines are also in very general use for medical purposes, and in the last few years there has been a still further application of magneto-electricity, namely, for the protection of our coast in time of war. We have all been exceedingly warlike within the last few years, and contrivances have been devised to prevent our coast being invaded, if it should ever be attempted. The most successful of these contrivances is the application of Faradaic electricity to the explosion of torpedoes. I have had a little torpedo made—not a very alarming one—and we will explode it in this room. Here is a little canister filled with powder. At the lower part of the canister is a fuse, which is ignited when a current of magneto-electricity passes across it. Now the difficulty in actual practice is to blow up the torpedo, containing a large charge of powder, exactly under a ship that might be approaching the coast. The difficulty has been overcome in this way. A number of these torpedoes are sunk when an invasion is expected, and their position is first marked by a little flag, and carefully noted. The observations are made by angular measurement, and the flags are removed. When an enemy's ship is approaching the coast the arrangement shown in the diagram is adopted. A telescope moving over a graduated circle has, you see, a wire passing along its axis. That wire leads to a submarine wire joined to the fuse of the torpedo, and passes back to another observer, who is provided with a similar telescope, but whose position is a mile or two further along the coast or river. In the circuit of the wire is, you observe, a magneto machine that will explode the torpedo if

the circuit be complete. But at present the circuit is not complete. You see there is a little gap at both stations, under the control of both observers: both those gaps must be closed before any current can pass. Now let us suppose the enemy's vessel is seen by one observer, whom we will call A: he knows where the torpedo nearest the vessel is laid. He turns his telescope to that spot, and awaits the moment when the vessel will cross his line of sight; instantly it does so, he presses down the key which closes the gap in the circuit at his end. But what is the observer B doing? He has received a signal to look out for this vessel, for there is an ingenious means of signalling along the torpedo wire without igniting the fuse. The observer B turns his telescope to the spot where the torpedo is laid. Now it is clear A sees along one line and B sees along another line; where these two lines intersect there is the exact spot at which the torpedo is buoyed. B now awaits the vessel crossing his line of sight; when it does so, he at once closes the gap at his end. Now, if A still sees the vessel in his line of sight, his gap remains closed, and as B also sees the vessel, it is obvious the vessel must be precisely over the spot where the torpedo is. This, then, is the critical moment. But what has happened whilst I have been describing the state of affairs? Both the gaps have been closed, the circuit has been completed, the current has passed, the fuse has ignited, the torpedo has been exploded, and the vessel is majestically rising into the air! We will now explode our model torpedo that is moored under a little ship in that distant tub of water. I hope all our connections are properly made, and that the torpedo will behave as it ought to do. [This it did not at first, but on a second trial it exploded with a sharp report.]

And now I must bring this lecture to a close, but before I do so I wish, by means of the lime-light, to show you some portraits of Faraday himself, and views of the place where he worked, and the rooms where he lived. The picture now on the screen is a photograph of Faraday taken not long before his death. It is a very faithful likeness of him. Here is a second photograph, in which he is holding a piece of heavy glass in his hand, the use of which substance you will not have forgotten.

The next view will enable you to see the spot where all his researches were made. This is the laboratory of the Royal Institution—by no means a sumptuous place: on the contrary, quite poor in comparison with other laboratories. But here it was Sir Humphrey Davy discovered the safety lamp. Here Dr. Thomas Young conducted some famous investigations.

Here Dr. Tyndall has so brilliantly worked. Here Dr. Frankland has made some important discoveries in chemistry. And here Faraday, like a Saul among the prophets, spent the greater portion of his illustrious career. These few square yards of an underground room have been the arena of some of the greatest exploits in science. Within the last six months nearly the whole of this place has been swept away, and more modern and convenient laboratories are being erected. The next photo graph is a view of Faraday's study, where he wrote out the results of his work below stairs. A few years before his death, Faraday had a small house at Hampton Court given him by the Queen; and in that cottage, a picture of which is now on the screen, he spent the last months of his life, and there he died. By his own wish he was buried in the simplest and most unpretending way, and a few words mark where he lies in Highgate Cemetery.

In quitting this subject it is hard to refrain from speaking of the noble character that made Faraday a hero worthy of our highest honour. But I trust you will not be satisfied with what you have heard to-night, but will turn to those biographies that have been published. A truer picture of Faraday has not been drawn than that with which Dr. Tyndall closes his fascinating and eloquent memoir. "In Faraday beauty and nobleness coalesced, to the exclusion of everything vulgar and low. He did not learn his gentleness in the world, for he withdrew himself from its culture; and still this land of England contained no truer gentleman than he. Not half his greatness was incorporate in his science, for science could not reveal the bravery and delicacy of his heart." And, I may add, in the noble counsel Mr. Ruskin gives to art students you will find the motto of Faraday's life: "Seize hold of God's hand, look full in the face of His creation, and there is nothing you will not be able to achieve."

CHRONOLOGICAL MEMORANDA RELATING TO THE LIFE OF FARADAY.

THE following summary may be useful to those who wish to have a chronological arrangement of the chief events in Faraday's life. In the foregoing lecture it was necessary to depart from such an arrangement:—

AGE.

Birth, at Newington Butts, London, September 22, 1791.
13.—Apprenticed to a Bookbinder.
20.—Attended some of Davy's Lectures at the Royal Institution.
21.—Made his first Electrical Experiments.
22.—Appointed Chemical Assistant at the Royal Institution of Great Britain, March 1, 1813.
„ Travelled on the Continent with Sir H. Davy for two years.
24.—Gave his first public Lecture, "On the Properties of Matter."
25.—Published his first Contribution to Science.
„ Minor Investigations for four years.
29.—First Paper in Philosophical Transactions; discovery of new compounds of Carbon and Chlorine.
„ Appointed Superintendent of the Laboratory, after being seven years Davy's Assistant. (End of Early Scientific Education.)

30.—Marriage to Miss Barnard, June 12, 1821.
31.—Discovery of Electro-Magnetic Rotation.
32.—Discovery of Liquefaction of Chlorine and other Gases (whence greatest Artificial Cold, &c.)
„ First Honorary Title awarded, elected Member of the Paris Academy of Sciences.
33.—Elected F.R.S. January 1824.
34.—Appointed Director of the Laboratory.
„ Discovery of Benzol and other Hydro-Carbons (whence Aniline Dyes, &c.)
36.—Began Christmas Lectures to Juveniles.
„ Experiments on Making Optical Glass for four years (whence Heavy Glass).
38.—Appointed Lecturer at Woolwich Royal Academy.
39.—Experiments on Optical Illusions and Vibrating Plates.

AGE.

40.—Commencement of Electrical Researches, August 29, 1831.

" Discovery of Current-Electric Induction, October 1, 1831 (whence the Induction Coil and its associated effects).

" Discovery of Magneto-Electric Induction, October 17, 1831 (whence Magneto-Electricity, employed for Private Telegraphs, Lighthouse Illumination, Electro-Plating, Military and Medical Purposes, &c.)

" Discovery of the cause of Magnetism by Rotation.

41.—Discovery of Terrestrial Magneto-Electric Induction.

42.—Researches on Identity of Different kinds of Electricity, 1833.

" Researches on Electric Conduction.

" Researches on Electro-Chemistry: Discovery of Laws of Electrolysis, 1834.

" Appointed Fullerian Professor of Chemistry.

44.—Accepted Pension from Government.

" Discovery of Electric Current induced in selfsame wire, the so-called Extra-Current.

45.—Appointed Scientific Adviser to Trinity House, in 1836, holding this post nearly thirty years.

" Researches on Frictional Electricity for three years, from 1836.

" Discovery of Specific Inductive Capacity (whence application to Submarine Cables).

49.—Researches on the Origin of Electricity in Voltaic Battery, 1840; and also previously in 1834.

50.—Illness and enforced rest for three years.

53.—Awarded the highest scientific honour, elected one of the eight Foreign Associates of the Paris Academy of Sciences. In all, Faraday received 95 titles or honorary distinctions, and was elected a member of 72 learned societies.

54.—Researches on Magnetism for 10 years, from 1845.

" Discovery of the "Magnetisation of Light," September 13, 1845.

" Discovery of Diamagnetism and the Magnetic Condition of all matter, November 4, 1845.

55.—Researches on the Diamagnetism of Solids, Liquids, and Gases for three years.

59.—Researches on the Relation of Gravity to Electricity, 1850, also in 1859.

" Researches on "Atmospheric Magnetism."

60.—Researches on Radiant Magnetism—"Lines of Force."

" Discovery of "Regelation" (whence theory of Glacier Motion).

62.—Exposed popular error of "Table-turning," 1853.

64.—Conclusion in 1855 of 24 years of published Electrical Researches; 30 series of Papers, containing 3,430 weighty paragraphs.

65.—Researches on the Relation of Gold and other Metals to Light, filling 300 folio pages of Laboratory Notes, 1856.

71.—Last investigation, on the Action of Magnetism on various Spectra, 1862.

74.—Relinquishes all Work and appointments, 1865.

76.—Death at Hampton Court. August 25th, 1867.

"His life was gentle; and the elements
So mix'd in him, that Nature might stand up
And say to all the world 'This was a man!'"

ANCIENT AND MODERN EGYPT;

OR,

THE PYRAMIDS AND THE SUEZ CANAL.

A LECTURE

By Dr. W. B. CARPENTER, F.R.S.,

President of the British Association.

Delivered in the Hulme Town Hall, Manchester, February 19th, 1873.

It gives me, I assure you, very great pleasure to meet you again in this hall. Since we last met, I have endeavoured to imitate the good example of my friend Professor Roscoe, by inaugurating a series of Science Lectures for the People in London; and, I am happy to say, upon even a greater scale than these. For last night I addressed an audience of about two thousand in the Town Hall of Shoreditch, giving them the second of two lectures on the Researches recently made into the Physical Conditions and Animal Life of the Depths of the Sea, which, you will remember, formed the subject of one of my lectures to you last year. I was encouraged in this undertaking by the great interest and attention manifested by the large audiences I addressed here; for I ventured to believe that the same subject would be interesting to a similar audience in London. I began with some misgivings; for I found that at this north-east end of London there never had been any lectures of the kind; and these doubts were shared by my friend Mr. Hansard, the Rector of Bethnal Green, who has taken so active an interest in the great Museum recently established there, of which you have all heard, and who kindly undertook the local arrangements for these lectures. We scarcely ventured to expect an audience of above 500 on the first evening; but we had above

2,000; and this number has steadily maintained itself through the course,—thus giving encouragement to those amongst us who are desirous to place the results of high scientific investigations within reach of our working brethren. I say "working brethren," because *we* work in our way. I think I may say that addressing an audience last night of 2,000 persons in London, after a long day's work—coming down to Manchester, and giving a lecture at the Royal Institution this afternoon—and coming here this evening to address this large audience—is a pretty fair twenty-four hours' work. I say that this appreciation of our efforts on the part of the people is a great encouragement to us, who, occupying such positions as I have the honour to hold at the present time, may call ourselves without arrogance the leaders in science. It is a great encouragement that we can count on this sustained interest and attention, amongst the very intelligent class who have hitherto been in a great degree debarred from receiving that culture which is to be derived from the addresses of men who have made scientific subjects their special study.

In my previous lectures to you on "Researches in the Deep Sea," and in the "Depths of the Human Mind," I spoke upon subjects which had been my special study. To-night, I have chosen a subject, with the concurrence of Dr. Roscoe, which is not so strictly *scientific* in its character, although by no means destitute of scientific interest. The matter to be treated is rather one of current knowledge, relating to scenes with which we are all more or less familiar by description, but which came under my personal observation during a few days' visit to Egypt. My remarks will be illustrated by a series of photographic views of the ancient monuments of Egypt, which convey a most faithful representation of them.

I was very strongly impressed in the course of my short visit with the different feelings conveyed to the mind by the two great works of modern and of ancient times—the Suez Canal and the Pyramids of Egypt. Taking the Pyramids as the type of the great ancient monuments of Egypt, the contrast was great between them, both in point of utility and picturesqueness. The utility of the Suez Canal is, I believe, only just beginning to be appreciated; but, as regards its appearance, it is about as ugly a thing as can be imagined; for nothing can be more ugly than a long and broad ditch, perfectly straight for thirty miles; so that when we came at last to a slight bend in it, I found the change quite refreshing. The contrast was heightened when, on the next day, I saw that remarkable city, modern Cairo, and those wonderful ancient monuments,

the Pyramids; and the impressions produced by this contrast between two of the most ancient and modern remarkable products of human labour, is one that will not be easily forgotten to the end of one's life. My object to-night is, to convey to you some of the impressions I myself received.

In the first place, let me direct your attention to the Physical Geography of Egypt; because, to understand what Egypt is, and the part it has performed in the history of the world, you must have a general idea of its local peculiarities. You all know the geographical position of Egypt—that it forms the junction between the two great continents of Asia and Africa, and is at the mouth of that wonderful river, the Nile, as to whose source we are still in a great degree of doubt —doubt which we hope will be resolved by the further researches of Dr. Livingstone. Now the Nile runs for a very long course indeed between ranges of hills. Here is a large map of modern Egypt, showing the Red Sea, the Isthmus of Suez, the Peninsula of Sinai, and the course of the Nile, the southern boundary of Egypt being at about the first cataract, above which is Nubia. From Assouan (the ancient Syene) down to Cairo, a distance according to the course of the river of nearly 500 miles, the river runs between two ranges of hills which are seldom more than ten miles apart, and it is between these ranges of hills on either side of the river, that the cultivation of the land is carried on under circumstances most favourable to extraordinary productiveness. I shall presently advert to this when speaking of the Delta. During this long passage of the river Nile between these hills, there is not a single stream running into it. Why? Because there is no rain, or next to no rain, through the whole year round in Egypt. There has been a little more than usual this season, which you know has been an exceptionally wet one. I have now a son in Egypt, who has been up as far as Thebes; and he reports that they have had a much colder winter than usual, with occasional showers and a good deal of wind, which sometimes brought dust-storms from the desert. But as a general rule there is scarcely any rain in Egypt from one year's end to another; therefore, you will see that there can be no rivers excepting the great river Nile which flows from sources very far south. Instead of rain, however, they have the fertilising influence of fresh water, derived from the Nile in a manner of which I shall presently speak. At Cairo this ridge of hills comes to an end—"tails out," as we say; and it is just on the end of the ridge, on one side, that the upper part of Cairo, including the great Mosque and the Citadel, is built; whilst on the other side of the Nile, on the end of this ridge, are built the Pyramids. Now here

was a fact for which I was not at all prepared. I had a notion that the Pyramids stood on the general level of the desert. They do not. They are built upon the tail of this ridge, which is there from 120 to 150 feet above the Nile; therefore, after coming along the causeway which is built as a passage over the overflowed country (for when I was there it was still partially inundated), we had to go up the hill to get to the Pyramids, and there was a sort of cliff from the base of the rocks down to the river. Now, below Cairo, the country is a perfectly dead level all the way to Alexandria and Port Said; there is no hill the whole distance, and scarcely any undulation of the ground. [Dr. Carpenter again referred to the map, to show the position of the Suez Canal.] There is the Gulf of Suez, and there is Cairo, and there you see an extensive tract of what we call "alluvial" land; that is, land made up of the fine particles washed down by the water of the river, which gradually settle down, constituting what is called the Delta, from its resemblance to the Greek letter D turned upside down, thus ∇. This same term is applied to similar formations at the mouths of other rivers, as for example the Ganges, the Amazon, the Mississippi, &c. An ancient writer on Egypt says that the portion of land below Cairo—that is, the Delta—is the "gift of the Nile." In order to give you an idea of the way in which the land grows and changes, I shall direct your attention to the difference between the ancient and the modern mouths of the Nile. Here is a map of ancient Egypt, which is exactly the same, in all its great physical features, with modern Egypt; but there is a marked difference in the distribution of these mouths. In ancient times, one of the principal mouths was called Pelusian, from the ancient town of that name. This mouth was an important feature in the ancient geography of Egypt, and its existence led to the earliest attempt to connect the Red Sea through the Nile with the Mediterranean. You will see marked here a canal which was cut by one of the Pharaohs—the canal of Necho, leading from the modern Suez into this Pelusian branch of the Nile. This seems to have been sufficiently large for the ships of those days; for it is recorded that after the defeat of Antony and Cleopatra at the battle of Actium, it was proposed that the Egyptian portion of their fleet should take refuge in the Red Sea, by making use of this channel of communication from the Mediterranean. The Alexandria mouth of the Nile was very small in comparison. I want you to observe the great size relatively of the Pelusian mouth of the Nile in this map of *ancient* Egypt; whilst, when you cast your eyes over the map of *modern* Egypt, you see that in the

position of the ancient Pelusian mouth there is a very narrow stream. In fact, there is no harbour here at all, it is entirely "silted" up; that is, closed up by the earthy accumulations brought down by the river, and washed back from the Levant. Then, on the other hand, the Damietta and Rosetta mouths have increased, and the main stream of the Nile finds its way into the Mediterranean by those mouths. The Alexandria mouth is not much increased. This shows that the Delta is undergoing changes in its physical features, in consequence of changes in the flow of the water in different parts; the blocking up of one mouth causing an increased rush of water to pass through the others, which enlarges them and keeps them clear.

A feature that struck me as we coasted along from Alexandria to Port Said, was that even in a vessel of no very great size (drawing about sixteen feet of water) we were obliged to keep several miles out to sea; because the water deepens so very slowly, that at four or five miles from shore there was not a depth of twenty feet. This, it was thought by Robert Stephenson, would prove an insuperable obstacle to the success of the Suez Canal. Stephenson believed that the quantity of muddy deposit continually accumulating would prevent the mouth of the Suez Canal from being kept open; as there is, of course, no flow of water as in the bed of the Nile. That danger of the choking up of the mouth of the canal by the back-wash from the Levant was pointed out, but it was not deemed insurmountable by other engineers. The manner in which they have attempted to prevent such accumulation is this: they have carried out from the harbour of Port Said two long breakwaters or piers, to a considerable distance seaward, hoping that by the accumulation of the deposit on the *outside* of these piers, which are nearly parallel, the waterway between would be kept open; and up to the present time certainly that hope has been realised, for there has been no trouble in keeping this centre way clear. It strikes one as remarkable that vessels have to keep four or five miles from the land, before they can enter the mouth of the canal. The line of entrance is marked by lighthouses and guide-towers, for guidance by night and by day. It is only in this line, which has been excavated by powerful dredges, that entrance can be effected; and this excavation of the sea-bottom is really a continuation of the canal.

When we come to enter the canal, the prospect is by no means charming. You see a long, low, level spit of land, on which Port Said is built; and all the rest of the shore merely encloses a

shallow lake—just covered with water—the land not being quite high enough to prevent the sea overflowing. It is, in fact, a great sandy swamp, through which the canal has to go for a considerable distance. Another apprehension was, that where the canal passed through this mass of alluvial soil—partly sandy and partly clayey—the sides of the canal, when excavated, would come together, and the bottom would rise, from the weight of the loose earth on either hand; just as happens when you cut into a peat bog. It is commonly supposed that this results from the *growth* of the peat—the plants which form the peat continuing to grow, and filling it up. This is not so, or is only so in a very small degree; but happens simply because the peat has a certain viscousness, something like thick treacle; for it is not a solid substance; and hence there is no sufficient resistance to prevent the sides from coming together; and it was apprehended that this would be the case with the Suez Canal.

Now it happened to me to go through the canal with the officer who surveyed it for our Admiralty at its opening—Captain Nares, who is now the commander of the "Challenger" expedition. As we went along, he took soundings of the canal constantly, to determine its depth. There was a very large troop-ship about to follow us, and we knew that it was just a question of a foot—no more—whether this ship could get through the canal or not. He found that the bottom had not risen in the least degree during the two years that the canal had been open. (It was a year and a half ago that I visited the canal.) If in those two years the bottom of the canal had surged up to the extent of only one foot, that great troop-ship would not have been able to go along it; therefore the depth was an important point to determine. He found, to his great satisfaction, that there was not the slightest change in the depth of the canal; and the troop-ship followed us safely.

I now come back to Port Said, where we saw a long low strip of land, almost on a level with the water, with a number of wooden buildings, one of which bore the grand title, "Hotel de l'Europe." Well, I suppose it *is* the Hotel de l'Europe, in the sense that the natives of every country in Europe occasionally stop there; but it is not at all an inviting place. We had only to take in a little coal, and I had not the curiosity to go ashore, the place looked so uninviting. There was not the least appearance of a garden or vegetation of any kind. We then entered this long ditch, extending 30 miles in a straight line. The whole canal is about 90 miles long from Port Said to Suez; but part of the canal runs

through a series of lakes about midway; especially the great Bitter Lake, through the deeper part of which it was not necessary to make any excavation, because the water was already deep enough, the level of this Bitter Lake having been previously a good deal below that of the Mediterranean and the Red Sea. One of the first points in laying out the canal was to determine whether there was any difference of level between the Mediterranean and the Red Sea, which had been reputed to be the fact. But this was found not to be the case; any small difference of level now and then observable being due very much to prevalent winds. Thus, when there is a strong westerly wind, the water will be driven up into a corner of the Levant, and will therefore rise at the Mediterranean end of the canal; and if there is a strong southerly wind the water will rise in the north at Suez, and then the Red Sea will be a little higher. Excepting these variations, there is no difference between the level of the water in the Mediterranean and the Red Sea. But this Bitter Lake had a level considerably lower, for it was like the Dead Sea in the fact that the evaporation was very much greater than the water it received. And how it received water it was difficult to say, unless by a sort of percolation of the water of the Nile through the loose soil of the bed of the lake. The water of this Bitter Lake is excessively salt even now, though the canal has freely admitted the sea water into it. I may mention that I found the sea water along the coast of Egypt to be very much less salt than usual, in consequence of the large quantity of fresh water brought down by the Nile. But although the canal has brought in fresher water from the Mediterranean as well as the ordinary water of the Red Sea, still the water of the Bitter Lake contains nearly twice as much salt as ordinary sea water.

As we went along the canal, we passed between mounds or banks, higher than the ordinary level. These banks were composed of material which had been excavated from the canal and thrown up on either side. As we steamed along the canal very slowly (for no vessel is allowed to go more than about four miles an hour for fear of injuring the banks of the canal), I mounted the "bridge" of the steamer so as to be able to look over this bank, and there I saw this interminable barren waste on the Egyptian side covered with water, and on the eastern side a sandy desert extending to Palestine. One of the first features of interest was to come upon a "floating bridge," thrown across the canal by steam, at the point which I was told was the track of the caravans. Now here was a most curious conjuncture of modern and ancient civilisation. This caravan track is one of the most ancient of all.

roads, leading from Egypt into Palestine and Syria, on the very line along which Jacob's sons may have gone down into Egypt to buy corn; and there we found one of the appliances of modern civilisation in the shape of this "floating bridge," which consisted of a large vessel, connected with two chains which lie along the bottom of the canal, and which are wound and unwound upon large drums by a steam engine, causing the large flat-bottomed boat to cross and re-cross the canal. This contact of ancient and modern civilisation is one of the most remarkable features in Egypt. I mentioned just now that my son had been ascending the Nile as far as Thebes; and he reports a fact which I was not aware of—that a railway now runs a good way up the Nile, so that, to our surprise, we received letters from him posted from time to time as he ascended the river. He tells us that he saw steam ploughs, made in Leeds, working side by side with the plough that has been in use for at least 4,000 years. He also saw sugar factories with the latest improvements in the machinery for making sugar direct from the cane, by means of the centrifugal apparatus, such as is employed at Bristol, where the beautiful white crystal sugar is made from the imported brown sugar; but in Egypt it is made direct from the sugar-cane juice. The culture of the sugar-cane was probably introduced into Egypt from India long before the Christian era, and the primitive Egyptian sugar-making apparatus worked by the natives carries one back at least 2,000, and perhaps even 3,000 years. The native Egyptians, Arabs, and Turks, may be seen squatting down at their prayers on their little carpets in the middle of the boiling-house, while the operations are going on, according to the Mohammedan custom.

This contact of ancient and modern civilisation was one of the first features of interest which struck me. But there was another noticeable feature. There are stations all along the canal, at which the officers reside, as well as the men who keep watch over the canal, and are ready to help if any vessel gets into trouble by grounding, also to insist that the regulations of the canal are maintained. For instance, no vessel is allowed to go on at night, but has to be fastened to posts fixed in the banks of the canal. At certain points there are sidings, where vessels pass each other. These things have to be looked after by the guardians of the canal. At every one of these stations I noticed that there was a garden, generally with a gay show of flowers, and a great cultivation of esculent vegetables. Now what was the meaning of this? How could these gardens be made out of this sand and mud? The secret is, that every one of these places is supplied with

fresh water. Now where does that fresh water come from? It is brought all the way from the Nile; for the first thing necessary before the construction of this canal was to bring *fresh* water in a canal from the Nile, there being no fresh water to be got from Port Said to Suez—nothing but brackish water, obtained by digging. This was very unpleasant to drink, as well as unfit for the boilers of the locomotive engines on the railway; for the use of which a reservoir was made at Suez, and filled by "water trains," carrying water all the way from the Nile at Cairo. Well, the present arrangement is this: a *fresh-water canal* has been cut from the Nile at Cairo to a place called Ismalia, a sort of half-way house along the Suez Canal, at one end of a pretty lake about three miles long, surrounded by slight hills—altogether a very interesting place after you have been passing through this monotonous straight ditch. At Ismalia this fresh water canal joins the salt water canal. The fresh water canal is not much used for vessels, and those only small ones. But pipes convey the fresh water to the railway which runs from Suez to Ismalia, and thence along the line of the fresh-water canal to Cairo. This, as you see by the map, is a great distance for the railway to go round; but the detour is made instead of crossing the desert, where there would be no water to be got, and no passengers to pick up. At these railway stations, by the aid of this supply of fresh water, anything can be grown in luxuriance; nothing being wanted for the soil in that sunny clime but water. At Ismalia, Mons. Lesseps, the head engineer, has a villa, with the most beautiful plants of all kinds, those of tropical as well as of temperate climes, growing luxuriantly in his garden. The other officers of the canal have also villas and gardens, less elaborately cared for, but very pretty. Then all around this little town of Ismalia there were patches of cultivation, resembling very much what we call "allotment" gardens in the neighbourhood of our towns. I suppose that any-body may go and take a piece out of the desert, and turn it into a garden, if he chooses to take the trouble; all that is required to bring the desert under cultivation being to dig a little extension of the ditch to bring fresh water from the canal. The effect was very curious. I could trace the successive stages of cultivation, from the first and rankest kinds to the most cultivated. At first, merely reeds would grow, then plants of a less inferior kind, but requiring nothing but sand and mud. These rougher and fibrous kinds served by their decay to make a vegetable mould; so that in a few months the ground would be ready for a crop of beans, or some other esculent plant, which would leave a good deal

of stalk to bind the soil together; thus preparing it for a still better crop.

This suggested an analogy in human life—that it is the duty of all of us to cultivate our life-soil in the best way we can, so as to leave it better for the next generation; the life of one generation producing that which gives greater *thinking power* to those who come after us.

These were some of the most interesting features which I witnessed in this short journey through the first part of the canal, and then across the desert into the cultivated portion of Egypt. I should mention that all these maps are coloured, so that the yellow shall represent the desert, and the green the cultivated portion; and you see how exceedingly narrow is the strip of green above Cairo; but then you must remember it is 500 miles long, and that the land is about the most fertile on the face of the earth. For thousands of years these branch canals have conveyed fresh water from the Nile to fertilise the land of this long narrow strip, as well as of the Delta, to which it has been applied by primitive machines worked by men, donkeys, or oxen. Thus by the aid of the constant sunshine, and of the natural and artificial irrigation two or three crops a year are obtained from nearly all the land of Egypt. You see in this map the network of canals over the Delta, which dates from an early period of the Egyptian monarchs. It is partly by the annual inundation of the Nile, and partly by the system of artificial irrigation, that the land is made to possess this extraordinary fertility.

In the Delta of the Nile, sugar, cotton, and various grains grow in luxuriant abundance. What struck me was the miserable dwellings of the poor; but since, owing to the climate, the poor spend little time in their dwellings, and often sleep outside, that might account for their very fragile and unsubstantial character. I was not struck with any apparent wretchedness or squalor in the appearance of the people. They seemed to be comfortable, and looked well-fed, and were not so unhappy as they had been described to me. There was no doubt these poor people were kept down in the most extreme poverty; but we must also bear in mind that a little food goes much farther in a climate like that of Egypt than in England. When I was at Malta, the year before, I took pains to inquire as to the earnings of the men, and found that those employed in discharging the coal ships into barges—which is very hard work—might earn 2s. per day, or 12s. per week. Upon that they had to keep a wife and family—and the Maltese generally have very large

families. I accounted for the men being able to support their families on this sum, by the circumstance that in a warm climate less food is required than in a cold one—a large part of our food being required to keep up the heat of the body. I have been assured by engineers engaged in constructing Indian railways, that a Hindoo on a pound of rice daily, and now and then a little bit of fish and butter, will do two-thirds of the work of an English navvy, with his six and eight pounds of beef a day, with bread and cheese besides. (Loud laughter.) I do not pretend to say that I have had an opportunity of going carefully into the subject; but my impression was that there was not as much 'surface appearance of great physical misery as I expected to see. You must not understand me to be defending the system of enforced labour. There is no doubt that system is a most terrible thing. The Khedive takes any quantity of men from their families, merely giving them something to eat, and does not give them wages; so that their wives and children are left to get a livelihood as best they can. This is a most abominable system. The land is in the possession of the Khedive, and has been in the hands of the Ruler through all changes of sovereigns, from the time of Joseph, when the Pharaoh of that time got the land into his possession. There is no middle class in Egypt—no class of landed proprietors. There are only the Khedive and the cultivators of the soil, and the necessary overlookers.

One word more in regard to the Suez Canal. There is, I believe, no question amongst those who have studied the subject carefully, that this route (if the company, to use a common expression, do not "cut their own throats" by imposing rates too high) will become, in the course of eight or ten years, the regular highway to the East for all our commerce. The distance through the canal to Point de Galle, for instance, the most southern part of India, is very little more than half what it is round the Cape of Good Hope. Of course no *sailing* vessel could take this route with advantage, because the uncertain winds in the Mediterranean and the Red Sea would be almost sure to cause a long detention; but *steamers* that can make their way against all winds, unless extraordinarily violent, can make the Suez passage with great certainty. What is necessary is that steamers should be built of such dimensions as to fit them, as it were, to the canal; and that is now being done upon a very extensive scale. Those who are likely to know best, say that in a few years there will be very few ships going round by the Cape of Good Hope, unless the price of coal should remain so high as to destroy the profit of the shorter route.

What I have to say with regard to the *Ancient* Monuments of Egypt will be best said in connection with the individual illustrations; and to these, therefore, we will now proceed.

The photographic pictures were then thrown upon the white screen. The first had nothing very picturesque about it, but was of interest, as showing the entrance to the Suez Canal and the dredging apparatus for excavating it and piling up the banks, just as in making a railway embankment. The second view showed the lighthouse at Suez, with the two long piers running out to the sea. No. 3 represented the starting of the fleet at the opening of the canal. One portion of the fleet went up the canal from Suez, and another portion came in from the Mediterranean; and they met at Ismalia, where the rejoicings took place. Dr. Carpenter said he saw the villa, with its splendid ballroom, which the Khedive built there, but which was now going to ruin. Another view was shown of the marine procession; and this was succeeded by a view of an Arab encampment; numbers of Arabs and Bedouins of the desert, who never come near a town under ordinary circumstances, having gathered from all parts to witness the opening of the canal.

Cleopatra's Needle at Alexandria was next shown. This remarkable monument was strikingly depicted in the photograph. It is believed to be one of the most ancient monuments in Egypt. It stands in a builder's yard at Alexandria, as though no value were attached to it—a strange place for so interesting a monument. On one side the sculpture is in a remarkable state of preservation; but on the side exposed to the prevailing wind of the desert, the inscriptions are almost obliterated by the abrading action of the fine sand through countless generations.

A view of Cairo showed how rapidly it is becoming modernised by the present sovereign, who seems to be ambitious to make Cairo as much like a French city as possible. The Pyramids were visible in the distance. The view, Dr. Carpenter said, was exactly like the one visible from his hotel. The Pyramids were about eight miles distant. Even then their height impressed him very strongly, not only because they stood upon an elevation of the ground, but because the slight haze of the atmosphere, or aerial perspective, still further magnified their proportions and altitude. The Citadel of Cairo, with the Great Mosque or Moslem church, was shown next. The mosque was adorned with two lofty and graceful minarets, of which those at the Alhambra, in London, are feeble imitations.

The next view was a street in Cairo, a very effective picture, the Eastern architecture being thoroughly characteristic, particu-

larly the projecting balconies, which are usually painted of a bright colour. The balconies have lattice-work outside, to prevent the street passengers perceiving the ladies within. From the gallery of the minaret is uttered the call to prayer, in the loud, ringing voice of the muezzin. A picture of one of the tombs of the early Caliphs gave occasion to Dr. Carpenter to remind his auditors of the poor lunatic mentioned in the New Testament who had his dwelling among the tombs; the explanation being that it was customary to erect buildings over the burial places of distinguished persons, and these were not unfrequently turned into houses by the destitute. Other views of mosques and the city of Cairo were shown, the flat tops of the houses presenting a curious appearance. Dr. Carpenter mentioned that he had lately seen a photographic picture of Jerusalem, which appeared to have been taken just after a washing day; for on the flat roof of almost every house clothes were hanging out to dry and bleach. The next picture showed the interior of a curious old mosque, called the Fountain of Ablutions. On a certain day in the Mohammedan year, the Khedive and all the great officers of State proceed to this fountain to wash away the sins of the past year. The building exhibited some interesting features of Arab architecture, being one of the oldest Arab buildings in Egypt. The Arabs have superseded the ancient Egyptian population, who are only represented by the Copts, a poor, down-trodden people. More pictures of mosques were shown, exemplifying different styles of architecture. The Pyramids were then shown and described. Dr. Carpenter praised the views for their fidelity to nature, a quality which photography possessed to perfection. These pictures brought the Pyramids before him exactly as they appeared when he saw them. He described their construction, appearance, size, and the character of the limestone rock of which they are made. Last year he had spoken to them about the chalk now being formed at the bottom of the Atlantic, by minute animals developed in countless numbers. The whole of the rock *upon* which the Pyramids are built, and *of* which they are built, with the exception of the granite casing which at one time existed, and which only exists in part on one of them now—is formed by what geologists call Nummulitic Limestone. This is newer than the old chalk, and is made of the shells of animals called Nummulites—like little pieces of money about the size of a shilling. There were vast beds of this rock where the Pyramids were built; and out of this rock was also formed the Sphinx, which seems to have been carved out of a mass of living rock left standing where the

enormous mass of stone had been taken away from around it for the construction of the Pyramids.

The Pyramids are nearly solid; there being only a few small chambers in them, used as burial places. According to Herodotus, each king began to build a Pyramid at the commencement of his reign, the centre or core of the Pyramid being built as a sepulchral chamber, and covered with one layer of stone, and a fresh layer of stone being added for every year of his reign. The great Pyramid of Cheops was, therefore, the sepulchre of the king who reigned the longest. There are 300 or 400 of these pyramids in the valley of the Nile, the largest being of enormous and surprising magnitude. The base of the great Pyramid covers an area as large as Lincoln's Inn Fields, in London; and it towers far above the top of St. Paul's Cathedral. The original height was 480 feet; but the top has been removed, and it is now about 450 feet high. There are 150 courses of stone, each stone being, on an average, three feet high. At a distance of two or three miles these courses of large stones look like courses of bricks; but on getting nearer the gigantic size of the structures is very impressive. Unfortunately, he had not time to make the ascent. It is said that some of the Caliphs removed great quantities of stone from the Pyramids to build their mosques; there being good reason to believe that the sides of the Pyramids were formerly "faced" with a smooth layer of granite brought from a long way up the Nile. There is no longer any trace of this covering on the Great Pyramid; but it is still perfect on the upper portion of the second Pyramid—that of Cephrenes.

Other views of the Sphinx were shown. Dr. Carpenter stated that an Arab offered him a bit of the Sphinx, which he had broken off. Of course he refused it, and thought such depredations should be punished. A fine series of monumental views were next shown from the temples of Philœ, Abou Simbel, Dendera, Edfou, and Karnac. These stupendous and beautiful erections were intended to immortalise their founders. There appeared to have been a superfluity of labour in ancient Egypt, which the kings employed in the erection of these gigantic pyramids and temples, the vastness and beauty of which were still impressive after the lapse of thousands of years. The sculptures on the massive columns were often in a remarkable state of freshness, and these sculptures depicted, not only the histories and military exploits of the kings, but the daily life of the people. Dr. Carpenter pointed out the great size of the blocks of stone used in these temples, and the admirable character of the work-

manship; the stones in most cases being fitted together with astonishing nicety, so that a knife could hardly be thrust between the joints. Then there was a dignity and repose about the sculptured figures which was very impressive; and so thick were the massive columns in some of these temples, that their gloom must have been appalling. There was nothing in history more interesting than to trace the life of this remarkable people from the sculptures on their monuments. These sculptured representations give us not only the language of the Egyptians, and the occupations of their ordinary life, but also their ideas of a future state. There is thus preserved what they call the "Book of the Dead," which gave the history of the soul and its judgment.

It is extraordinary, added Dr. Carpenter, to see how large a part of the *later* Hebrew ideas on these subjects is derived from Egyptian sources. You know that the Mosaic dispensation put aside altogether the idea of a state after death. There is no trace in the law of Moses of any future judgment. We only find doubtful references to it in the middle period of the Hebrew monarchy; but it came to be a current idea among the later Jews before the time of Christ.

Now it is most singular to observe that the Egyptians should have held this idea of a future state so strongly, and yet that Moses should not have introduced it as a part of his religion. Mr. Zincke, one of Her Majesty's chaplains—a man with a large amount of liberal culture and a deeply religious spirit, has applied himself to these great problems in an honest and true manner, and fully recognises the circumstances I have mentioned. That clergyman says: "How was it that, with this striking recognition of the future state by the ancient Egyptians, Moses did not introduce it into his religious system? Why was is it so completely ignored in what we are accustomed to call the Mosaic economy?" Mr. Zincke comes to the conclusion at which I know many sagacious thinkers had previously arrived, that Moses felt that here was a people trodden down by slavery, without any higher culture, for four hundred years. They were nothing better than children, and it would be generations and generations before they would grow up into anything beyond the gratification of their immediate desires. If we read the whole of that singular history of the Jewish commonwealth, we should see that what was always on their minds was this: that because they had done *this* ill they were punished, and because they had done *that* well they were favoured. Their feeble ideas could not range beyond the present; and therefore Moses, with the judgment and wisdom of a great man—learned in all the

culture and wisdom of the Egyptians—refrained from presenting that which he knew they could not take in, and presented to them that which they would take in. History shows us how constantly national transgression was followed by national punishment, and we may feel sure that will be the case with us. Ill-doing is always followed—at some time or other, and in some way, in the great dispensations of Providence—by punishment in this world; and every good and well-directed effort has in the end that success which it deserves. That success may come in ways we do not dream of, but the result of good influences which he knew to have been sown fifty years ago by very humble individuals, has been seen in the end. He had the greatest faith in the progress of humanity, if each would only seek to do his duty in that state of life in which it had pleased Providence to place him.

It would have been of no use to try and influence the Hebrews by motives connected with a *future* state; and the only religious motive which was capable of influencing them, had reference to *present* rewards and punishments. What the Ten Commandments say as to rewards and punishments, has reference to the temporal prosperity—"Honour thy father and thy mother, that thy days may be long upon the land which the Lord thy God giveth thee." The more carefully we examine the history of the Jewish people after they got into the Promised Land, the more distinctly does it appear that they were entirely destitute of any distinct notion of a future state.

Now it is most remarkable to see, that not only this belief, but the language in which it was expressed in the ancient Egyptian times, anticipated that of the Christian Revelation. For in this "Book of the Dead" there are used the very phrases we find in the New Testament in connection with the day of judgment. The whole proceedings of the Judgment are distinctly set forth; and when an individual spirit makes his claims to merciful judgment, he is represented as saying: "I fed the hungry; I gave drink to the thirsty; I clothed the naked; I visited the sick." Now these phrases are unquestionably made use of in the "Book of the Dead," which was engraved probably two thousand years before the time of Christ. There is a curious addition put in by one claimant, who says—"I did not make long speeches." I am afraid I have made you a very long speech to-night. I thought it well to close my lecture with these remarkable statements, because there is no doubt about them. All who have studied the Egyptian records are perfectly agreed that these are the simple facts of the case. Nor do they in any way detract from the value or authority

of the Christian dispensation; but they show that similar ideas were entertained by a great people two thousand years before. How that idea came to fade out is a very remarkable question. How that great Egyptian civilisation came to pass from the earth, and to leave no trace behind but these monuments and sculptures, is a question which has puzzled the wisest heads. But there is the simple fact.

I may mention just one other—that on the roof of the temple of Dendera there is a most curious picture of the Creation, which clearly embodies very much of the idea conveyed in the first chapter of Genesis. The sun is shining down; the waters are disappearing from some parts, so as to make a separation between land and water; the striking of the fervent rays of the sun on the land makes plants to spring up, and there are numerous animals appearing in different parts; so that, altogether, it gives that kind of pictorial representation that many very good critics have regarded as the source of the narrative given in the first chapter of Genesis.

Many of what may be called the liberal school of Biblical critics have come to suspect that this record was a sort of translation of pictures into verbal language; so that it expressed the early Jewish ideas, based upon the pictorial records of the Egyptians among whom they lived. That I believe is a very probable account of the origin of the narrative of the Creation. All unprejudiced Biblical critics have now come to accept these narratives, not as truthful historical statements, nor, on the other hand, as forgeries, but merely as expressions of the early belief of the people whose sacred literature they constituted. The form and mode of creation there expressed is all subordinated to the one great idea—"In the beginning God made the heavens and the earth;" and whatever we may think as to the precise historical value of these records, we must agree that the more they are investigated by scientific minds the more we come to feel that they cannot be received as more than what I have said—a record of the early ideas of this very ancient people. When we trace that record back, we find it reaches much further than the origin of the Jewish people; for there can now be scarcely any question that the ancient Egyptian civilisation carries us back thousands and thousands of years before the time that the Jewish people came into Egypt. Thus, you see, we have in these wonderful records a clue to the growth of Mind among the most ancient civilised people of whom we have any knowledge.

These are a few of the thoughts to which the contemplation of

these remarkable works leads us; and I have placed them before you as my own free and independent convictions, without, of course, in any way binding or pledging any one else to them. But I can assure you that they are the views which intelligent and able thinkers in all religious denominations are now arriving at; and my firm belief is that they will come to be generally diffused in the next century. The revelations of science as to the history of the earth and its successive inhabitants can no longer be screwed and twisted into conformity with a set of writings, which, however ancient, can only be taken as representing the beliefs of the ancient people whose sacred literature they constituted; and it is not by the beliefs of a people so low in the scale of culture as not to be able even to apprehend the doctrine of a future life, that the beliefs of the highest intellects and most religious natures of the present time are to be trammelled.

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